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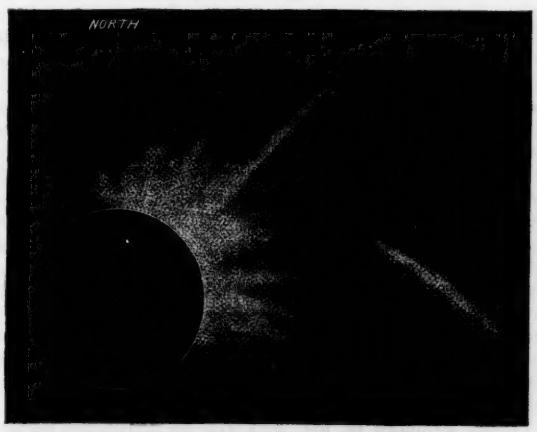
THE COMET SEEN DURING THE ECLIPSE.

BY A. C. RANYARD.

THE accompanying sketch gives the position of the comet, as shown on my photographs, with some of the more marked rays of the corona stretching out from the sun's limb on the side towards the comet; but no attempt is made to indicate the smaller structure of the corona. The scale of the wood-cut nearly corresponds with



the largest of the photographs. It will be seen that the head of the comet is situated at about a solar diameter from the sun's limb, and that the tail is greatly inclined to the line joining the head of the comet with the sun's centre. There is a slight curvature of the comet's tail; but its general direction is such that if a medial line, which we will call the axis of the tail, were produced, it would not pass through the moon's disc. It is evident that this inclination of the tail cannot be merely due to an effect of perspective, the tail of the comet being really radial, and the nucleus situated on this side of the sun's centre, or on the opposite side of the corona—for a straight line can only be projected into a straight line or a point; and if the sun's centre lay upon the axis of the comet's tail produced, the tail, from whatever position it was viewed, would always appear as radial to the sun's limb. A little consideration will show that the inclination of the tail to the line drawn from the sun's centre to the head of the comet may have been greater than it appears in the photograph, but it cannot have been less.

Though the fact that the tail of the comet was greatly inclined to the radius vector struck me on first examining the photographs, the probable significance of the fact has only recently occurred to me. Small jets issuing in various directions from the nuclei of large comets have frequently been observed, but these jets do not extend to any great distance from the nucleus; and, as far as I am aware, they have not been observed extending beyond the outer envelopes of the head of the comet; while the chief tail into which the envelopes of the head merge streams away in a direction opposite to the sun. If the comet were moving freely in space (that is, not in a resisting medium), and a repulsive force from the sun was the only force acting upon the matter of the tail, we should expect to find the tail either straight, or curved in the plane of the orbit of the comet, when the velocity with which the matter of the tail is driven away is very great, compared with the orbital velocity of the nucleus, we should expect to find a straight, or very nearly straight, tail stretching away from the sun; but if the velocity of the matter of the tail is not very great, as compared with the orbital velocity of the nucleus, we should expect to find a curved tail, with the part in the immediate neighbourhood of the nucleus directed away from the sun, for the matter of the tail on leaving the head of the comet would have an orbital velocity which would carry it onward equally with the nucleus, if undisturbed by a resisting medium, and the small particles of the tail would only drop behind as the motion of the nucleus and particles of the tail were changed by the central forces acting upon them. But if the comet were moving in a resisting medium, the small particles of the tail would lose their orbital velocity more rapidly than the matter of the nucleus, and the tail would have the appearance of being blown backwards by a wind.

Having regard to the direction of the curvature of the tail, which is slightly concave towards the south, and to the inclination of the general axis of the tail to a radial line drawn from the sun's centre to the nucleus of the comet, we may, I think, assume that the comet was moving in an upward direction, but whether towards us or away from us, and whether it was nearer or more distant from us than the sun's centre, or whether it was coming up to or had passed perihelion, must, I fear, remain a mystery.

The comet observed by Sir I. Newton in 1680, and the comet of last year which was referred to by Mr. Proctor as having not improbably had its period shortened by the resistance of the corona, both, probably, passed within a third of a radius of the sun's surface. From general reasoning one would suppose that under the influence of solar gravity the density of the gaseous atmosphere of the corona would increase rapidly as we descend from the outer limits of the corona towards the photosphere, but our want of knowledge with respect to the temperature of the coronal area and the behaviour of dust particles and gaseous matter at such high temperatures prevents our making any useful assumption with respect to the law of increase of density of the solar atmosphere. It is known that in the chromosphere at the base of the corona, hydrogen, and many of the terrestrial elements, give a spectrum of narrow lines similar to the spectrum

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given in terrestrial laboratories under very small pressures; but, whatever the density of the gaseous matter of the chromosphere may be, there are facts which render it probable that there is a resisting medium within the coronal area which increases in density as we descend from the outer limits of the corona towards the photosphere. The structures in the lower parts of the corona are more curved than the rays which extend into the outer parts of the corona, as if they had been acted upon and bent by currents in an atmosphere, and the contorted forms of some of the irruption prominences also indicate the existence of a resisting medium. Definite evidence with respect to the existence of such a resisting atmosphere has been deduced from observations made by Professor Young with respect to the rate of projection of irruption prominences. See a paper by Mr. Proctor, published in the *Monthly Notices* of the Royal Astronomical Society for December, 1871.

The comet observed during this eclipse is not the only cometary body which has been observed in the neighbourhood of the corona during an eclipse. The photographs taken by Mr. Davis and Col. Tennant, during the eclipse of 1871, show an opaque nucleus, with dark concentric structures partially surrounding it, which cut out the light of the bright parts of the corona, and seem to have been due to a small comet situated between the earth and the corona. An account of these dark structures is given in the *Monthly Notices* for June, 1874. Professor Winnecke, in describing the corona, observed during the eclipse of 1860, also speaks of a dark body which appears to have been unconnected with the corona; he describes it as a parabolic dusky arc, which looked as if it was drawn with sepia upon the brighter background of the corona. A drawing of this curious body is given in the memoirs of the Imperial Academy of St. Petersburg, 7th Series, Vol. IV.

THOUGHT-READING.

BY THE EDITOR.

IN the cases last considered, the explanation may be suggested that some code of signals may have been arranged by which the proper answer was communicated to those questioned. The following cases are free from this objection:—

"It will be well to give," writes Prof. Barrett, "a group of results obtained when no member of the family was aware of the selected object. Eleven times running we chose a card at random, and on six of these occasions one of the children named the selected card (giving both suit and pips, or fully designating the court card) correctly at the first trial; twice the card was named correctly on the second trial; and three cases were failures. On none of these occasions was it even remotely possibly for the child to obtain by any ordinary means a knowledge of the card selected. Our own facial expression was the only index open to her; and even if we had not purposely looked as neutral as possible, it is difficult to imagine how we could have unconsciously carried, say, the two of diamonds written on our foreheads. The outline of results during the present investigation, which extended over six days, stand as follows:—Altogether, 382 trials were made. In the case of letters of the alphabet, of cards, and of numbers of two figures, the chances against success on a first trial would naturally be 25 to 1, 51 to 1, and 98 to 1, respectively; in the case of surnames they would, of course, be indefinitely greater. Cards were far most frequently employed, and the odds in their case may be taken as a fair medium sample; according to which, out of the whole series of 382 trials, the average number of successes at the first attempt by an ordinary guesser would be $7\frac{1}{3}$. Of our trials, 127 were successes on the first attempt, 56 on the second, 19 on the third, making 202 in all. On most of the occasions of failure, 180 in number, second trials were made; but in some cases the guesser professed inability, and declined to make more than one, and in others we allowed three; no trial beyond the third was ever allowed. During the last day or two of trial, after it had occurred to us to notice the point, we found that of the failures to guess a card at the first trial, those wrong both in suit and number were a small minority. Our most striking piece of success, when the thing selected was divulged to none of the family, was five cards running, named correctly on a first trial; the odds against this happening once in our series were considerably over a million to 1. We had altogether a good many similar batches, the two longest runs being eight consecutive successes, once with cards, and once with names; where the adverse odds in the former case were over 142 millions to 1, and in the latter something incalculably greater. If we add to these results others obtained on previous visits, it seems not too much to say that the hypothesis of mere *coincidence* is practically excluded."

"The exceptional nature of this inquiry," proceeds Professor Barrett, "goes far to invalidate arguments founded on character and demeanour; and, on this head, we will only state our conviction that any candid critic, present during the whole course of the experiment, would have carried away a far more vivid impression of their genuineness than the bare printed record can possibly convey. Of more real importance is the hypothesis of exalted sensibility of the ordinary sense organs. We could discover no indication of this in any of its known forms; but by way of precaution, as has been already stated, we commonly avoided even whispering any word, number, or name that we had selected; and the position of the excluded child, when the door was opened, would in every case have satisfied the most exacting critic. The explanation which might be sought in unconscious indications given by the sitters, and especially in the movement of the lips, has been already adverted to. Coming as we did to this investigation with considerable previous experience of the same kind, we were throughout strictly on our guard against giving such indications ourselves; the possibility of their being given by the family was, of course, excluded where the family were ignorant of the selected word or thing; and on the remaining occasions our perpetual vigilant watch never detected a trace of anything of the kind. The absolute docility of the children—both the guesser and the others—in taking any position in the room that we indicated, was naturally an assistance to our precautions. It may be further mentioned that, on a previous visit made by one of us, the child called the required name through the shut door, or from an adjoining room, having thus been completely isolated from the very beginning to the very end of the experiment."*

Other evidence of this sort will be considered further on. At this stage it may be well to note the objection raised by Professor Donkin. "The matter in question,"

* "Among the friends above referred to as having taken part in these inquiries are Professor Balfour Stewart and Professor A. Hopkinson, of Owen's College. A communication lately received by us from them, embodying the results of their visits, and written without any knowledge of the contents of this paper, states facts and instances criticisms as to the possible (or impossible) relation to those facts of *coincidence*, *collusion*, *sight*, and *hearing*, precisely similar to those we have given. Their experience was that 'in about half the cases the first guess was right, and in most cases of mistake there was some marked point of similarity between the object proposed and the thing guessed.'"

he says, "has obtained a somewhat undue prominence of late; but if it is as simple and intelligible as it appears to be to most who have investigated it with care, and with minds free from mystical bias, any aid towards the extinction of what must then be regarded as an *ignis fatuus* of pseudo science carries with it its own justification." Passing over cases in which there was actual contact between the persons guiding and guided, Professor Donkin remarks that in cases where there was no actual contact, "common sense demands that every known mode of explanation of facts should be exhausted before the possibility of an unknown mode is considered." "It is equally obvious that in all scientific inquiries the good faith of individuals concerned should form no part of the data on which the conclusion is to rest. We can never call on science to put deception out of court by a belief in any one's integrity. Half of the evidence which has propped up the spiritualistic craze is based on the results obtained through mediums of "unblemished character" in private families, whose virtuous reputation has been largely sustained by the fact that they did not take money for their trouble; no regard being paid to innumerable other motives and tendencies to deception." (This is very well put.) He then considers the "code of signals" explanation, which "fully serves to cover all the facts in question," though it is only by straining the evidence that the cases in which no members of the family were present when an object was selected, that Professor Donkin makes out this point. "From the only rational point of view," he says, "that of scientific scepticism, and, therefore, with total disregard of the personal factor, this consideration seems in no way to invalidate the line of comment here taken. It is not clear to how many of the three observers the pronoun 'we' in the passage [above] refers, but, at any rate, we miss entirely in the paper any specific quotation of results obtained in the latter set of circumstances. But even if this evidence had been forthcoming, no mere *ipse dixit* on such a matter could for one moment be admitted. Reason would require us to entertain the great probability of mental bias in some, at least, of the observers, or to discredit the accuracy of their memory, rather than to allow that anything has been adduced in this account of what (to say the least) must be regarded as superficially-conducted experiments, to warrant a recognition of any novelty, or by consequence to stand in need of explanation by a theory of 'brain-waves.'"

The spirit of extreme caution here indicated is altogether sound; the objection to novelty, as such, is as entirely unsound. *Nothing* could prove that mind acts on mind if Professor Donkin's principle were accepted in its full extent. The theory might be established so far as he himself was concerned, by an experience of his own, but no one else would be bound to accept it, and it cannot possibly be proved to each person separately and individually.

Professor Donkin seems unaware of the fact that Dr. Carpenter, who has dealt with such subjects more closely perhaps than any living man of science, and always from the sceptical side, admits all that, as I conceive, even Professor Barrett and his colleagues consider proved. In the following passage the reader will note the distinction between what Dr. Carpenter has been led to suspect, and what he regards as beyond question :—

"Everyone who admits that 'there are more things in heaven and earth than are dreamt of in our philosophy,' will be wise in maintaining a 'reserve of possibility' as to phenomena which are not altogether *opposed* to the laws of physics or physiology, but rather *transcend* them. Some of my own experiences have led me to suspect that the power of intuitively perceiving what is passing in the mind of another, which has been designated as 'thought reading,' may, like certain forms of sense perception, be extraordinarily exalted by that entire concentration of the attention which is characteristic of the states we have been considering. There can be no question that this divining power is naturally possessed in a very remarkable degree by certain individuals, and that it may be greatly improved by cultivation. So far, however, as we are acquainted with the conditions of its exercise, it seems to depend upon the unconscious interpretation of indications (many of them indefinable) furnished by the expressions of the countenance, by style of conversation, and by various involuntary movements; that interpretation, however, going, in many instances, far beyond what can have been learned by experience as to the meaning of such indications."* "Looking at nerve force as a special form of physical energy, it may be deemed not altogether incredible that it should exert itself from a distance, so as to bring the brain of one person into direct dynamical communication with that of another, without the intermediation, either of verbal language, or of movements of expression. A large amount of evidence, sifted with the utmost care, would be needed to establish even a probability of such communication. But would any man of science have a right to say that it is *impossible?*"

(To be continued.)

A STUDY OF MINUTE LIFE.

By HENRY J. SLACK, F.G.S., F.R.M.S.

A SPECIMEN of bottom yeast—the *unterhefe* of the Germans—taken from a vessel in which fermentation had been arrested for some time, showed, under a magnification of 200 linear, that the little plant cells had been starved. All of them were much too small, and instead of being approximately globular, many were sausage-shaped, some like thin cylinders, and amongst them many minute beaded rods, which would be ranked amongst the *Bacteria*. Any further fermentation carried on under such circumstances would give rise to products not wanted in any process of brewing or wine-making, and, if nothing worse happened, a good deal of vinegar would be formed at the expense of the alchohol which resulted from the action of the true yeast cells. Beer or wine in that condition is popularly said to be pricked. Pure diluted alcohol has no tendency to undergo a fermentation into the vinegar condition, but if any of the microfungi of the vinegar-plant sort are present, and also some easily oxidable vegetable matter, the acetic change soon occurs. One of the best modes of making vinegar on a large scale is to cause a fluid containing alcohol and sweet-wort to trickle slowly over wood shavings or birch twigs. A slimy substance forms in a few days all over the shavings or twigs, and this modifies the alcohol into acetic acid. If the slimy stuff is examined under the microscope, it is found to contain, besides yeast-like cells, millions of small beaded rods—a kind of bacterium. Both brewers and wine-makers are much troubled through

* Dr. Carpenter then mentions some very curious examples related in the autobiography of Henrich Zschokke, who (according to his own statement) possessed this power in a very remarkable degree, frequently being able to describe, not only the general course, but even many particulars, of the past life of a person whom he saw for the first time, and of whose history he knew nothing whatever.

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these and various microfungi they do not want intruding into their fermentations, and giving their liquids a bad flavour. In other very numerous cases the beer or wine seems all right at first, but will not keep. Whether in cask or bottle, some of the intrusive ferments continue in operation, either producing vinegar, or some other substance with an objectionable flavour. To minimise the danger, brewing should be carried on in pure air, and with the greatest regard to cleanliness. Fermenting the wort in a warm temperature, as is common in this country, is more favourable to the growth of the undesirable organisms than the cooler process so much employed in Germany. If beer, made by "high fermentation," that is at a warm temperature, and with the yeast cells rising to the top, is intended to keep for a length of time, or to stand a sea voyage, it must be strong and well hopped, the aromatic oil of the hop being unfavourable to the growth of microferments. Besides getting sour, beer and wine are liable to turn ropy and putrid, through the action of special ferments whose germs are very widely diffused. Slender beaded organisms, straight, curved, or spiral, some quiet, others in quick motion, preside over these changes, producing what Pasteur calls various "maladies" in the fluids they attack. The "low fermentation" goes on slowly at low temperature, and the yeast falls to the bottom of the vessel. The temperature should be about 45° F., and great quantities of ice are used in the German breweries to enable the process to be carried on during the warmer months. The beer is also kept in cool cellars, and Pasteur states that nearly 2 cwt. of ice is consumed in getting 22 gallons of beer ready for sale. If well made and carefully bottled, this kind of beer will keep without being of great alcoholic strength, or highly hopped.

If wine is likely to be affected by the intrusive ferments, Pasteur recommends bringing it near the boiling point after it is put into bottles. This kills the microferments, and, if carefully performed, leaves the wine with a good flavour, but it is said that when so treated it does not improve with age.

It is interesting to let a saccharine fluid spontaneously pass into the acetous fermentation and examine the organisms under the microscope. If a little yeast is used to start the alcoholic fermentation, and the liquid is then left for some time in a warm place and exposed to the air, it is pretty sure to turn sour. From a wide-mouthed bottle containing an ounce or two of a saccharine fluid so treated a little ropy mass is taken from the bottom and found to be composed of vast multitudes of minute beaded threads thickly interlaced. Sometimes a leathery mass is found—the so-called "mother of vinegar," or vinegar plant. Many families make their own vinegar by its means. They put it into a pretty strong solution of coarse brown sugar, and in a few weeks find it is turned to good vinegar, and the plant greatly increased in thickness. It is then like a stout piece of wet buff leather, and easily splits horizontally. If a small piece is placed in a bottle such as is used for Preston salts, in a solution of lump sugar, a growth is soon obtained transparent enough for convenient examination. After viewing it with a low power, one of 400 or more diameters should be used. A number of yeast-like cells, associated with multitudes of beaded, rod-like cells, are then seen to be immersed in a mass of tough, gelatinous matrix. If a vinegar-plant is taken out of a vessel, with a sufficient depth of fluid to keep its under surface and part of its thickness completely submerged, and placed in a shallower one, so that it is much more exposed to air, it does not continue to carry on the acetous fermentation, but is soon covered with crops of blue mould. The sugar disappears, gets slowly burnt up, but neither vinegar nor alcohol is obtained. In some experiments by the writer, published in 1865 by the Microscopical Society, it was found that vinegar plants dried in an oven, at a temperature too low to burn them, were reduced to a state like dry gelatine, which clicked when thrown upon glass. Exposed to the air they absorbed moisture, and when thoroughly wetted the various cells appeared uninjured, but most of them no longer produced the acetous fermentation. In one case, the butyric fermentation occurred in a saccharine solution, evidenced by the peculiar and very nasty smell of highly rancid butter. In another case, after six months' immersion, a dried piece of the plant began to form vinegar, and produced loosely aggregated cells which carried on the process.

The same kind of cells may produce quite different effects under varying conditions. They may take the oxygen they want from the air, or, if that is excluded, obtain it by decomposing some substance present. The number of new combinations that may be produced by partial decompositions and oxidations is indefinitely large, and may give rise to "maladies" of beer, wine, butter, &c., and in the human organisms, to various diseases.

WAS RAMESES II. THE PHARAOH OF THE OPPRESSION?

By Amelia B. Edwards.

IV.—THE CONSECUTIVE AND SYNCHRONOUS THEORIES.

IT is difficult to make even the roughest historical calculation without some use of dates; but, as I have already said, no dates that can be applied to this early epoch of Egyptian history are other than approximate. We may estimate the number of years which elapsed between one given event and another given event, and we may thence calculate our way to a third event, and all three conclusions may be relatively accurate; but to label any one of the three with a positive date B.C. is, in the present state of our knowledge, impossible. Still it is necessary, if only in order to be intelligible, temporarily to accept a fixed date of some kind; and for this purpose I propose to accept Mariette's date, B.C. 1462, for the beginning of the XIXth dynasty. Mariette, it should be said, founded his chronological system on the historical lists compiled by an Egyptian historian named Manetho, who was high-priest of the Great Temple of Heliopolis and Keeper of the Archives, under Ptolemy Philadelphus, B.C. 284–246. Manetho, though a native Egyptian, wrote in Greek, which was the language of the Ptolemaic court (just as Turkish, and not Arabic, is now the language of the court at Cairo); and in Greek he composed his famous "History of Egypt," which was either a direct translation of ancient historical papyri, or an original narrative based upon those early chronicles. This invaluable work has perished; but some precious quotations from it are found in the writings of Josephus, Eusebius, and George the Syncellus before-mentioned. Above all, the great chronological list of dynasties and kings from Mena to Alexander is preserved entire in the works of several Christian chronologists. Following this list, Mariette places the XIXth dynasty between B.C. 1462 and B.C. 1288. The first three Pharaohs of this dynasty were Rameses I., Seti I., and Rameses II. Tabu-

lated according to the number of their regnal years, as recorded on the monuments, they stand as follows:—

Rameses I. reigned 6 years.
Seti I. reigned 51 years.
Rameses II. reigned 67 years.

To calculate these reigns would, at first sight, seem straightforward enough. If Rameses I. began the dynasty B.C. 1462, must not Seti I. have succeeded B.C. 1456, and Rameses II. B.C. 1405? And would not the sixty-seventh and last year of Rameses II. have fallen in B.C. 1339? And if we cast up the three reigns, shall we not produce a sum-total of 124 years?

Unfortunately, this is precisely what we may not do; and for the reason that Egyptologists, although agreeing as to the length of these various reigns, differ *in toto* as to whether they were consecutive or synchronous. If consecutive—each Pharaoh succeeding to the throne upon the death of his predecessor, in regular order—then the three reigns would undoubtedly represent a period of 124 years; but if synchronous—that is to say, if one Pharaoh began to reign during the lifetime of his predecessor, and counted his first regnal year from that date, instead of from the year when his predecessor died—then an entirely new calculation, resulting in a much-abridged total, will have to be made.

Fairly stated both ways, the question stands thus:—

1. The XIXth dynasty began with Rameses I., who reigned six years, and who may, or may not, have shared his brief authority with Seti I., his successor. But Seti I., who reigned for fifty-one years, undoubtedly shared his throne with Rameses II. for many years before he died; and Rameses II., more than half a century later, as certainly took his own successor, Menephthah, into the same kind of royal partnership. Now, according to the synchronous theory, which is Brugsch's theory, Rameses II. dated his regnal year from the time of his association with Seti I.; at which time Brugsch supposes him to have been about twelve or thirteen years of age. Some thirty, or thirty odd, years of his long reign of sixty-seven years would thus, it is maintained, be counted in with the reign of his father. If, therefore, accepting B.C. 1462 for the commencement of the XIXth dynasty, we allow, in round numbers, thirty years for the joint sovereignty of Seti I. and Rameses II., we bring the beginning of the younger Pharaoh's reign to B.C. 1435; his accession to the undivided crown to B.C. 1405; and his death, at the close of a reign of sixty-seven years, to B.C. 1368. Total of three reigns, from B.C. 1462 to B.C. 1368—94 years.

2. The advocates of the consecutive theory, as opposed to the advocates of the synchronous theory, found their argument upon the details of certain military campaigns which are chronicled in the form of sculptured bas-reliefs and graven inscriptions upon the walls of various temples in Nubia and Upper Egypt. Here, at battles and sieges distinctly dated as having taken place in the fifth and eighth years of his reign, Rameses II. is represented accompanied by several of his sons, who, though evidently mere youths, are at all events old enough to take part in the thickest of the fight. That the king is himself depicted in these tableaux as a man in the prime of life, cannot be accepted as evidence either way, since the artist would, of course, so represent him, no matter how old or how young he might be. But that a youth of seventeen or twenty should be the father of sons ranging from, at least, twelve to fifteen years of age, would be somewhat miraculous. If, however, we suppose the monumental dates of the reign of Rameses II. to be counted from the year of the death of Seti I., then Rameses II. would be thirty-five or forty years of age in the fifth year of his reign, and old enough to be the father of a large grown-up family. This, at all events, is the opinion of Maspero, of Lenormant, and of the majority of French and English Egyptologists. Starting, therefore, with B.C. 1462, for the beginning of the XIXth dynasty, the consecutive theory brings up the reigns of Rameses I., Seti I., and Rameses II. to a total of 124 years.

Beginning, as before, with the synchronous hypothesis, and holding fast to B.C. 1462, for the commencement of the XIXth dynasty, we will next see to what extent these dates correspond with the era of Nubti and the Hykshos calendar.

(*To be continued.*)

ENGLISH SEASIDE HEALTH-RESORTS.

By ALFRED HAVILAND, M.R.C.S., F.R.M.C.S. Lond.

CLASSIFICATION (*Continued from page 92*).

II.—AS TO THEIR POSITION ON THE SEABOARD; WHETHER THEY ENJOY THE WARM MOIST AIR FROM THE GULF STREAM, THE BRACING AIR OF THE NORTH SEA, OR AN ATMOSPHERE BROUGHT BY THE WINDS AFTER IT HAS LOST ITS SEA CHARACTER BY PASSING OVER THOUSANDS OF SQUARE MILES OF CONTINENT.

THE Gulf Stream, which has such a powerful influence on the climate, not only of our seaboard, but of the whole of the British Isles, must naturally claim the attention of all who really desire to acquire a sound knowledge of the cause of the exceptional high air temperature of our country within the interlatitudinal zones, described above, compared with that of other parts of the northern hemisphere lying within the same zones, but between different degrees of longitude to our east and west.

For the sake of clearness we may draw a distinction between the winter and summer temperatures of our atmosphere, inasmuch as their causes are distinct, although both are originally due to one grand cause—the sun. Our *winter heat* is an exotic, begotten by the sun-heat that pours down on the waters of the Gulf of Mexico, a large portion of which, as well as their point of issue, the Straits of Florida, lie under the tropic of Cancer. This sun-heat, thus conceived by the sea, is transported in a north-easterly direction, by the vast marine current of the Atlantic, which owes its origin to the south westerly anti-trade winds (our *true* south-west winds), to the Arctic regions, where, as it crosses the 80° of North latitude, its influence is still so great upon the atmosphere above it, between 70° E. and 10° W. long., as to create a mean annual temperature of 11° 22 Fahr., or an excess of 8° 96 over that of all other longitudes crossing the same parallel. This same sun-heat, of which the Atlantic is delivered along the whole course of the Gulf Stream, gives life, health, and vegetation to a large area of land, where, without its benign influence, the winter cold would kill, or make life difficult to the weak and diseased.

This vast volume of warm water, *the North-eastern* branch of which, after its separation from the *South-eastern* at 47° N. lat., and 25° W. long. flows to the north and east along the coasts of Ireland, Scotland, and Norway, between Europe and Iceland, and, as Haughton remarks, probably extends beyond Spitzbergen. The same author is of opinion that "this portion of the Gulf Stream owes its momentum to the *South-west antitrades*; while the other, or *South-eastern* portion, is deflected by the earth's rotation to the south-east and south, and flows southwards along

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the coasts of Spain and Africa until it rejoins the north equatorial current caused by the *North-east trades.*"

If we take the mean of the different estimates of the magnitude of the Gulf Stream, as given by Maury, Herschell, Colding, Croll, and Thompson, we shall find, according to Haughton,* that nearly 38 (37·834) *cubic* miles of warm sea-water are transferred from the tropics to the north temperate zone per hour. This vast mass of sea-water leaves the tropics at a temperature not less than 65° Fah., and is cooled down to at least 40° Fah. in the temperate zone. Maury estimates the rate of movement at five knots an hour, Sir John Herschell at four, Croll at four miles, and Sir Wyville Thompson† at three knots an hour.

We have seen that this enormous volume of sea-water transfers heat from the tropics to the north temperate zone, and then gives forth 25° of the 65° F. which it contained at its point of issue from the Straits of Florida.

By the warmth thus brought from the southern latitudes, the British climate is upwards of 20° *warmer*, writes Buchan,‡ in *winter* than it otherwise would be.

Haughton,‖ in his paper on the effects of Oceanic currents upon climates, published in the Report of the British Association for 1881 (York Meeting), has given much interesting matter on this subject. He there insists that the effect of the Gulf Stream upon our climate is limited to the winter, and that its effect on the summer temperature is *nil.*

For instance, he shows that the mean temperatures of January and July, the coldest and hottest months of the year, are as follows, taking the 50° N. latitude, which runs through the Land's End, Cornwall :—The *mean January* temperature at *Gulf Stream longitudes* is 40·00° F., whilst *at all other longitudes* it is 18·28° F., or 13·72° *below* freezing-point. The *mean July* temperature at *Gulf Stream longitudes* (Lat. 50° N.) is 63·80° F., whilst *at all other longitudes* it is 65·77° F., or lower by 1·97° F. ; and he concludes that the Gulf Stream adds nothing to the July temperatures of the localities affected by it, but rather, on the whole, has the effect of slightly diminishing the summer heat. The general climatal effect of the Gulf Stream is, therefore, to make the *annual range* of temperature less ; (the climate more insular), but it has no effect whatever upon summer heat or upon the fruiting of plants and trees that require a given July temperature for reproduction.

The effect of the Gulf stream on the winter temperature of our Seaside Health-Resorts is most marked, and by the kind permission of Mr. Alexander Buchan, M.A., F.R.S.E., whose able works on meteorology are now so well known and appreciated, we shall be able to illustrate this most important part of our subject by giving his charts of Isothermals, together with those promised in our last. It will then be clearly seen how each Health-Resort is affected, not only by the *winter warmth* of the sea, but by the *summer heat* of the sun—the one temperature being *exotic*, whilst the other is *indigenous.*

CRYSTAL PALACE. — It is proposed by the Crystal Palace authorities to organise an exhibition in the coming autumn and winter, which shall include both gas and electric lighting. This is an excellent move, and one in the right direction. The great difficulty with which the authorities will have to contend will, doubtless, be the mutual jealousies now so openly paraded.

* *Op. cit.*

† "The Voyage of the *Challenger:* the Atlantic," vol. i., p. 371. (Macmillan, London.) 1877.

‡ "Introductory Text Book of Meteorology," by Alexander Buchan, M.A., F.R.S.E. (Blackwood, Edinburgh.)

‖ p. 451.

HOME CURES FOR POISONS.*

VEGETABLE POISONS.

WE may leave mineral poisons for the present, to consider some which at the present season and during the next few months are more likely to be mischievous,—viz., vegetable poisons.

The most dangerous of these are the Hemlocks, including Hemlock Dropwort (*Ænanthe Crocata*), Water Hemlock (*Cicuta Virosa*), and the Common Hemlock (*Conium Maculatum*), Fool's Parsley (*Æthusa Cynapium*), Monkshood (*Aconitum Napellus*), Foxglove (*Digitalis Purpurea*), Black Hellebore or Christmas Rose (*Helleborus Niger*), Buckbean (*Menyanthes Trifoliata*), Henbane (*Hyoscyamus Niger*), Thorn Apple (*Datura Stramonium*), and Deadly Nightshade (*Atropa Belladonna*),—to which may be added, though unlikely to be taken in the same way, Cocculus Indicus (*Menispernum Cocculus*), and Tobacco (*Nicotiana Tabacum*).

The Hemlocks and Fool's Parsley are *Umbelliferæ*, and belong to the same order as carrots, parsley, celery, and parsnips. They owe their danger chiefly to their resemblance either in root or leaf to these kitchen-garden plants. Fool's parsley in particular has often been eaten by mistake for common parsley, from which, however, it may be readily distinguished by the black and shining surface of the under leaves, and their unpleasant smell when rubbed. All these four plants produce similar symptoms (Hemlock Dropwort in the most dangerous degree), viz., confusion of ideas, often terminating in delirium or stupor, coldness of the extremities, and occasionally nausea and vomiting.

Monkshood is a well-known garden plant, distinguished by its dark-blue, cap-shaped flowers, growing in clusters on a stem about three feet in height. The leaves are deeply divided, the upper surface dark, glistening green, the under surface pale-green. The symptoms produced are vomiting, convulsions, delirium, and stupor. There is generally noticeable swelling of the tongue and face, and tenderness of the stomach. The poison acts chiefly on the nervous system. According to Orfila, the juice of the leaves causes death in a short time; the root is still more energetic in its action.

Foxglove, though a wild plant, often finds a place in our gardens. Everyone knows its tall stems, and the purple flowers, shaped like a glove-finger (whence its German name, *fingerhut*). The leaves are dark-green, and rough. The symptoms of poisoning by this plant are lowering and intermission of the pulse, faintness, giddiness, and cold sweats, with hiccough and convulsions. The effect on the circulation is characteristic of the action of Digitalis; it has been known to lower the pulse from 120 to 50 in a few minutes. It also has a diuretic effect.

Black Hellebore is a dwarf plant, with deep-green leaves springing from the root. The flowers resemble those of the dog-rose in the number and shape of their petals. The symptoms are vomiting, giddiness, cramps, and convulsions.

Buckbean, which is often gathered with the common watercress, may be easily distinguished by the leaves, which are always trifoliate, whereas the leaves of watercress are divided into five or seven segments. Buckbean produces vomiting and colic, but seldom any serious mischief.

* It is perhaps hardly necessary to point out that for decoction of tincture of Quinine in our last paper on this subject (p. 71, line 7), decoction or tincture of *Cinchona* (or Peruvian Bark) should be read. The correction should be made *in situ*, as also a few lines further on, where Quinine is again, by a printer's error, substituted for Cinchona. The decoction may be taken in doses of a wineglassful, at intervals of from half an hour to an hour, or the powder itself may be used.

Henbane is common in hedges and by the roadside. The young shoots have sometimes been dressed for salad, and the roots have been mistaken for wild parsnips. The symptoms produced by this poisonous plant are dilation of the pupils, vomiting, purging, giddiness, delirium, and sometimes stupor.

Deadly Nightshade is commonly seen in our hedges, and known by its lightish green leaves, brownish purple flowers, and dark purple berries. The berries often prove attractive to children, and have been eaten with fatal consequences; but the leaves and roots are also poisonous. The following case, described by Orfila, gives a good idea of the usual symptoms: "One child ate four ripe berries of the *Belladonna*, and another six; an hour after, both committed such extravagant actions, that their mother was astonished; the pupils of their eyes were dilated, their looks were altered, and they appeared labouring under a cheerful delirium, accompanied by fever. The medical man called in found them in a state of great excitement, talking at random, running, jumping, and laughing convulsively, with purple countenances and accelerated pulse." Nausea and vomiting, generally without stomach pains, are noted; they seem to be caused by the gorged condition of the blood-vessels of the brain. Dryness of the throat is another characteristic symptom. But the most marked feature of poisoning by *Belladonna atropa* is dilation of the pupils of the eyes. The stomach is insensible to the action of ordinary emetics.

The same general treatment is to be adopted in all cases of vegetable poisoning; but in all serious cases it is essential that medical assistance should be obtained. Emetics (the sulphate of zinc, if procurable, but if not, the mustard emetic already described) should be used at once; the back of the throat tickled with a feather, and copious draughts of tepid water taken to excite and promote vomiting. Where these measures fail, the stomach-pump must be used. Neither ipecacuanha nor tartar emetic should be used to cause vomiting, as during the nausea they produce before vomiting is excited, the poison is more readily absorbed. Vinegar must not be given until the poisonous matter has been removed; but afterwards, it may be given in doses of a wineglassful, one part vinegar to two parts water, once every two hours in mild cases, but oftener—to half-hourly doses—in cases of greater severity. Where there is stupor, the patient should be kept walking about, and if the stupor is great cold water may be dashed over the head and chest. Strong coffee may be used where the narcotic effect of the poisoning is very marked. But as we have already mentioned, it is all-important that in cases of vegetable poisoning a medical man should at once be sent for; the messenger should be enjoined to tell the doctor the nature of the case, lest haply, instead of antidotes and stomach-pump, he should come mainly provided with surgical instruments. In the excitement of a sudden case of poisoning, a messenger is often simply sent for Doctor So-and-So, without being able to say for what purpose the doctor is so suddenly wanted.

(*To be continued.*)

ONE OF THE FIRST FRUITS.—The Markland Electric Light Company announce that the shares offered to the public not having been fully subscribed, the deposits paid on application will be returned at once without deduction.

UNDERGROUND WIRES IN NEW YORK.—The sub-committee appointed at Albany to report upon the practicability and feasibility of laying wires underground in cities in New York State have made a report in which they strongly urge the Legislature to pass a law compelling telegraph and telephone wires to be run underground.

JULY FLOWERS.

MIDSUMMER is a good time to master the plantains, for they may all be found now both in flower and in fruit together; and they are a curious family of plants, well worthy of close consideration. Probably everybody knows the typical plantain form by sight—in the rough at least; but as it is well to take nothing for granted, one may begin by saying that any roadside weed with a tall spike of minute flowers, each consisting of four greenish sepals, four thin, transparent, and colourless petals, united into a short tubular corolla, four stamens, and a pistil, is perfectly sure to be a plantain. They are all more or less exclusively wind-fertilised, and so they have long stamens hanging out to the breeze, and feathery styles to catch the pollen; and the styles always come to maturity first, being fertilised by the pollen blown from another head before the stamens uncoil themselves. We have five English species. Three of them have leaves about three times as long as broad, namely, *Plantago lanceolata*, *P. major*, and *P. media*. Of these, *P. lanceolata*, common ribwort, is by far the most abundant. It may be known by its long, narrow leaves, which rise from the ground nearly erect, as well as by its yellow stamens, and by the fact that each of its small, cup-like capsules contains only two seeds. When ripe, it divides in the middle, the top falling off like a cover, and shows the two little kernels within. This is the most degraded of our inland English species, having completely accommodated itself to wind fertilisation; its petals are a dingy grey or brown, and are tucked away inconspicuously behind the calyx. *P. media*, the hoary plantain, is far less abundant, and much prettier. Though also mainly designed for wind-fertilisation, it has not entirely ceased to attract insects; and so its petals are still a faint whitish lilac, its stamens are tinged with pink or purple, and it has a slightly fragrant almond perfume. It may be known by its close rosette of broad, silvery white leaves, pressed tight against the ground in a bunch, so as to kill off the grasses beneath it; as well as by the fact that its capsule usually contains four seeds, at least in the young state, though one or two of them sometimes wither away before ripening. This is by far the prettiest of our English species. *P. major*, the large plantain, is frequent along waysides. Its leaves are very broad, tapering into a stalk at the bottom, and less upright than in the ribwort. But the best distinctive mark is afforded by the capsule, which contains a number of very small seeds, as well as by the length of its spike, which is lower and longer than in the other species. This is also a very degraded kind, with small and very inconspicuous flowers, mostly concealed beneath their large green masking bracts. It is often given as food to canaries. The other two plantains are mainly seaside weeds. *P. coronopus*, the buckshorn plantain, has leaves which sub-divide into tines or branches, like a stag's antler, from which rough resemblance it gets its English name. Its tufted stock, with a ring of narrow, much-branched foliage, is familiar to everybody on cliffs and pastures by the seashore, as well as on sandy places inland. Its leaves lie flat against the earth, showing off their tracery-work most delicately on the pale background of a sand-patch. The flowers are very inconspicuous, with brownish stamens, and the calyx is fringed with minute hairs. There are four cells or divisions in the capsule, but the seeds seldom all ripen. *P. maritima*, sea plantain, is the least interesting of any. Its leaves are long and slender, but very fleshy, something like thick green cords; and its flowers are almost as minute as those of the buckshorn plantain. The capsules have only two seeds. This is the most

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degraded species of all, its flowers being quite unnoticeable, owing to their greenness or brownness, and it is apparently always wind-fertilised. Nevertheless, all of the plantains are originally descended from ancestors with bright petals and honeyed nectaries, as may be seen by their united corolla, which invariably points back to an earlier stage of insect fertilisation. *P. media* shows us best what is the sort of type from which these degenerate little weeds are ultimately derived. The others should be carefully compared with it, and their differences noted and accounted for. The key will be found in the relative adaptation of each kind to wind-fertilisation.

RISTORI'S LADY MACBETH.

ALTHOUGH the plan and purpose of KNOWLEDGE are inconsistent with regular dramatic criticism, the appearance of a great actor or actress in an important Shakespearean part may well be regarded as a matter within our scope; for if the actor should "hold the mirror up to Nature," as Shakespeare tells us, our great dramatist has depicted Nature herself, and the study of Nature is the aim of KNOWLEDGE.

If we study the work of great actors, and consider the secret of their success or failure, it will be seen how deep a truth underlies the trite saying quoted above. Just in proportion as the actor, granting him intellect to see what is natural, holds the mirror up to Nature, does he move his hearers. Each touch of Nature makes the whole audience akin with him. The gamut of feeling may extend beyond the range of most of them, just as the music assigned to a *primo tenore* runs beyond the range of ordinary singers; but even in scenes such as scarcely any can estimate from actual experience, truth to Nature can be as certainly recognised as the truth of a great singer's voice outside the ordinary range. Thus, if we analyse the feelings with which we follow a great actor or actress, we find that we are pleased or offended (for the best and greatest of them offend at times), just in proportion as they present truthfully or the reverse the action and character entrusted to them. This is true, despite what is also true, that on the stage the lights and shades of character must be intensified, in order that they may seem as strong as in real life. The scene-painter does not paint as a Claude Lorraine would paint; he uses devices such as a great landscape painter would reject: yet if he is a master of his art he aims at producing true effects as certainly as a Turner or a Claude. So it is, or so rather it should be, with the actor.

Yet so little is this recognised by the general run of actors, who, unfortunately, must always take a large part in the representation of every play, that, as a rule, it is painful to the real lover of Shakespeare to see any of his noble dramas acted. We believe that this, rather than any want of appreciation of the true value of Shakespeare's work, is the reason why, as an actor and manager of the day has said, Shakespeare "spells bankruptcy to the manager." It is, indeed, certain that all the Shakespearean plays require modification before they can suitably be represented on the modern stage; for the simple reason that many accessories which were valued in his time, and therefore suitable, are now useless, or even ridiculous. For example, although the witch scenes in Macbeth are inextricably interwoven with the plot, and, rightly arranged, might still be effective, it is certain that in their details they are now utterly unsuitable. No one is impressed when the first witch remarks that, "Like a rat without a tail, she 'll do, she 'll do, she 'll do; or when the three witches put into the cauldron a number of unpleasant ingredients. The more solemnly these scenes are given, the more do they detract from the effect they are intended to produce; so that, by the time Macbeth enters, we are very ill-prepared to be affected by his powerful conjuration. But in this and similar cases the fault is not Shakespeare's, but that of managers and actors, who cannot see how, with changing ideas, such scenes have become unsuitable. It was not here and thus that Shakespeare wrote "not for an age, but for all time." Where, however, his real power is shown and should be felt, nine actors out of ten fail to impress us with any feeling but a sense of incongruity. They address the audience, where speech and action should be addressed to each other; they mouthe, where they should speak "trippingly on the tongue"; they rant and rave, where they should be grave and reflective; they are violent, where they should be deeply moved; and in scenes where passion and fire are needed, they produce no effect on an audience already tired of their meaningless violence.

Madame Ristori, during the week ending last Saturday, brought into strong contrast the right and the wrong ways of presenting great Shakesperean parts. To begin with, she is a woman of intellect; and she has applied her intellect, in the first place to the study of Shakespeare's Lady Macbeth, and in the second to the adaptation of her own powers to the part. She has neither forgotten, on the one hand, that it is Shakespeare's conception she has to deal with, nor, on the other, that she can only in so far carry out his conception as nature has fitted her for the part. We do not say that her view of the part is, in our judgment, altogether correct. Occasionally she seemed to merge the ambition, resolution, and subtlety of Lady Macbeth, into an actually murderous temperament, of which there is no indication in Shakespeare's Lady Macbeth. On the contrary, Lady Macbeth shows early, and this indeed Madame Ristori represented admirably, that the effort by which she had compassed the thought of murder, had been too much even for her resolution. The anguish expressed by the actress while Macbeth tells her of his fears ("We have scotched the snake, not kill'd it," &c.) was admirably conceived, and justified, despite the resolute "But in them nature's copy's not eterne," by the tone in which Macbeth speaks of his plans. "Be innocent of the knowledge," he says, "till thou applaud the deed;" and again, "Thou marvell'st at my words; but hold thee still." The sleep-walking scene can only be rightly understood or rendered, when these subtle indications of Lady Macbeth's real character, and of Macbeth's recognition of it, are adequately noted.

There was one fault in Madame Ristori's presentation of the part, which seemed to us a grave one. Lady Macbeth has half persuaded her husband, in the speech beginning, "O, never shall sun that morrow see," to what he had half resolved on already. "We will speak further," is all he says; and seeing his face change into suspicion-breeding gloom, she answers—

> "Only look up clear;
> To alter favour ever is to fear.*
> Leave all the rest to me."

Madame Ristori closes this scene by a singular walk with Macbeth across the stage, in which nothing is said by either; but Macbeth, by repeatedly raising his hand, suggests that he is not convinced, while Lady Macbeth warningly raising hers, as often suggests that he must yield to her. This might do fairly well—though we doubt it—on a small stage; but continued across more than half

* Not "to be afraid," but to "cause fear," to "affright."

the width of such a stage as Drury Lane's, it has a decidedly ridiculous effect. We believe the close of this scene would be far more impressive if Macbeth retired moodily and doubtingly, watched by Lady Macbeth, as, anxious but resolute, she slowly follows him.

In all the remaining scenes Madame Ristori's acting, apart from somewhat too stately and Italian action in the banquet scene, was superb—the sleep-walking scene even more terrible, because more natural, than a death-scene by Bernhardt.

Madame Ristori speaks English singularly well, though with occasionally incorrect emphasis. Like all really great actors she knows when and how to speak quickly, reserving slow utterance for greater effect in the more impressive passages. Those around her on the stage, from the best to the worst, or rather from the worst to the least bad, seemed to regard the words belonging to their parts as opportunities for fixing upon themselves the attention of the audience, opportunities to be made the most of by dragging out the words as slowly as possible, emphasising every third word, and shouting or shrieking every third line. The effects of changes of modulation, of varying tones and rates of utterance, they seemed to think not worth trying. And that they were wise in their generation was shown by marked applause from the gallery. In most cases they followed tradition as to action and emphasis—where they departed from it they reached a lower depth. Macbeth's "Fill full," *soto voce*, as a hint that he wanted his own goblet well filled, is regarded we see by the *Athenæum* as a good idea, not as an absurdity. Macduff's despairing cry, "He *has* no children," becomes a feeble, rhythmless, explanatory remark, when emphasized "*He* has no children." But why note flaws, such as these, in what was full of faults? All that followed Madame Ristori's retirement after the sleep-walking scene (including her recall, by the way), was simply farcical—the fight between Macbeth and Macduff unworthy of Richardson's Booth.

BUTTERFLIES AND MOTHS.

By W. J. H. Clark.

A LARGER number of butterflies and moths are out this month than was the case in June, and most of those that were in full force then, still continue on the wing.

The Purple Emperor (*Apatura Iris*) is to be seen in the southern counties of England, and most of the "Fritillaries," the Red Admiral (*Vanessa Atalanta*), Painted Lady (*V. Cardui*), Marbled White (*Arge Galathea*), Greyling (*Satyrus Semele*), Small Copper (*Polyommatus Phlæas*), Green, Purple, and Black Hairstreaks (*Thecla Rubi*, *T. Quercus*, and *T. W.-Album*), and several of the "Blues" may now be found.

Among the moths, we may still find most of the "Sphingidæ" mentioned last month, and the Speckled Footman (*Eulepia Cribrum*), Four-Spotted Footman (*Lithosia Quadra*), and Common Footman (*L. Complanula*) are now on the wing.

The six Belted Clearwing (*Sesia Ichneumoniformis*), Wood Swift (*Hepialus Sylvinus*), Short-cloaked Moth (*Nola Cucullatella*), Muslin Moth (*Arctia Mendica*), Cinnabar (*Euchelia Jacobææ*), Brown Tail (*Liparis Chrysorrhea*), Gold Tail (*L. Auriflua*), Satin Moth (*L. Salicis*), Gipsy (*L. Dispar*), Black Arches (*L. Monacha*), Lackey (*Bombyx Neustria*), and Oak Eggar (*B. Quercus*) should all be sought for during the course of the month.

Among the Geometræ, we may expect to find the Swallow-tailed Moth (*Uropteryx Sambucata*), Bordered Beauty (*Epione Apiciaria*), Little Thorn (*E. Advenaria*), Early Thorn (*Selemia Illunaria*), Dotted Carpet (*Gloria Glabraria*), Mottled Beauty (*Boarmia Repandata*), Common Wave (*Cabera Exanthemaria*), Common White Wave (*C. Pusaria*), Clouded Magpie (*Abraxas Ulmata*), July Highflyer (*Ypsipetes Elutata*), Common Carpet (*Melanippe Subtristata*), most of the "Emeralds," and the Scalloped Oak (*Crocallis Elinguaria*).

The Noctuæ that fly this month are far too numerous to mention, but amongst others we may expect to find the following:—The Buff Arches (*Gonophora Dedrasa*), Peach Blossom (*Thyatira Batis*), Marbled Beauty (*Bryophila Perla*), Brown-line Bright-eye (*Leucania Conigera*), Common Wainscot (*Leucania pallens*), Dark Arches (*Xylophasia polyodon*), Bordered Gothic (*Neuria Saponariæ*), Straw Underwing (*Cerigo Cytheria*), Dot Moth (*Mamestra persicariæ*), Garden Dart (*Agrotis nigricans*), Gray Arches (*Aplecta nebulosa*), Marbled Clover (*Heliothis dipsaceus*), Golden Y (*Phisia Iota*), Burnished Brass (*P. chrysitis*), Broad Bordered Yellow Underwing (*Tryphœna fimbria*), Large Yellow Underwing (*T. pronuba*), and the Old Lady (*Mania Maura*).

On the 28th ult. I had a Death's Head Moth (*Acherontia atropos*) brought to me by a farm labourer, but I am sorry to say it was in a very damaged state, though still alive. Have any of our readers heard of this insect appearing so early in the summer?

FIRE RISKS FROM ELECTRIC LIGHTING.

THE following rules and regulations for the prevention of fire risks arising from electric lighting have been recommended by the Council of the Society of Telegraph Engineers and Electricians, in accordance with the report of the committee appointed by them on May 11, 1882, to consider the subject:—

The members of the committee were—Professor W. G. Adams, F.R.S., vice-president, Sir Charles T. Bright, T. Russell Crampton, R. E. Crompton, W. Crookes, F.R.S., Warren De la Rue, D.C.L., F.R.S., Professor G. C. Foster, F.R.S., past president, Edward Graves, J. E. H. Gordon, Dr. J. Hopkinson, F.R.S., Professor D. E. Hughes, F.R.S., vice-president, W. H. Preece, F.R.S., past president, Alexander Siemens, C. E. Spagnoletti, vice-president, James N. Shoolbred, Augustus Stroh, Sir William Thomson, F.R.S., past president, Lieutenant-Colonel C. E. Webber, R.E., president.

These rules and regulations are drawn up not only for the guidance and instruction of those who have electric lighting apparatus installed on their premises, but for the reduction to a minimum of those risks of fire which are inherent to every system of artificial illumination. The chief dangers of every new application of electricity arise mainly from ignorance and inexperience on the part of those who supply and fit up the requisite plant. The difficulties that beset the electrical engineer are chiefly internal and invisible, and they can only be effectually guarded against by "testing" or probing with electric currents. They depend chiefly on leakage, undue resistance in the conductor, and bad joints, which lead to waste of energy and the production of heat. These defects can only be detected by measuring, by means of special apparatus, the currents that are, either ordinarily or for the purpose of testing, passed through the circuit. Bare or exposed conductors should always be within visual inspection, since the accidental falling on to or the thoughtless placing of other conducting bodies upon such conductors might lead to "short circuiting," or the sudden generation of heat due to a powerful current of electricity in conductors too small to carry it. It cannot be too strongly urged that among the chief enemies to be guarded against are the presence of moisture and the use of "earth" as part of the circuit. Moisture leads to loss of current, and to the destruction of the conductor by electrolytic corrosion, and the injudicious use of "earth" as a part of the circuit tends to magnify every other source of difficulty and danger. The chief element of safety is the employment of skilled and experienced electricians to supervise the work.

I. The Dynamo Machine.

1. The dynamo machine should be fixed in a dry place.
2. It should not be exposed to dust or flyings.
3. It should be kept perfectly clean and its bearings well oiled.
4. The insulation of its coils and conductors should be perfect.
5. It is better, when practicable, to fix it on an insulating bed.
6. All conductors in the dynamo-room should be firmly supported, well insulated, conveniently arranged for inspection, and marked or numbered.

II. The Wires.

7. Every switch or commutator used for turning the current on or off should be constructed so that, when it is moved and left to itself, it cannot permit of a permanent arc or of heating, and its stand should be made of slate, stone ware, or some other incombustible substance.

8. There should be in connection with the main circuit a safety fuse constructed of easily fusible metal which would be melted if the current attain any undue magnitude, and would thus cause the circuit to be broken.

9. Every part of the circuit should be so determined that the gauge of wire to be used is properly proportioned to the currents it will have to carry, and changes of circuit, from a larger to a smaller

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conductor, should be sufficiently protected with suitable safety fuses, so that no portion of the conductor should ever be allowed to attain a temperature exceeding 150 deg. F.

N.B.—These fuses are of the very essence of safety. They should always be enclosed in incombustible cases. Even if wires become perceptibly warmed by the ordinary current, it is a proof that they are too small for the work they have to do, and that they ought to be replaced by larger wires.

10. Under ordinary circumstances complete metallic circuits should be used, and the employment of gas or water pipes as conductors for the purpose of completing the circuit should in no case be allowed.

11. Where bare wire out of doors rests on insulating supports it should be coated with insulating material, such as india-rubber, tape, or tube, for at least two feet on each side of the support.

12. Bare wires passing over the tops of houses should never be less than 7 ft. clear of any part of the roof, and they should invariably be high enough, when crossing thoroughfares, to allow fire escapes to pass under them.

13. It is most essential that the joints should be electrically and mechanically perfect. One of the best joints is that which is whipped around with small wire and the whole mechanically united by solder.

14. The position of wires when underground should be efficiently indicated, and they should be laid down so as to be easily inspected and repaired.

15. All wires used for indoor purposes should be efficiently insulated.

16. When these wires pass through roofs, floors, walls, or partitions, or where they cross or are liable to touch metallic masses like iron girders or pipes, they should be thoroughly protected from abrasion with each other or with the metallic masses, by suitable additional covering; and where they are liable to abrasion from any cause, or to the depredations of rats or mice, they should be efficiently encased in some hard material.

17. Where wires are put out of sight, as beneath flooring, they should be thoroughly protected from mechanical injury, and their position should be indicated.

N.B.—The value of frequently testing the wires cannot be too strongly urged. It is an operation, skill in which is easily acquired and applied. The escape of electricity cannot be detected by the sense of smell as can gas, but it can be detected by apparatus far more certain and delicate. Leakage not only means waste, but in the presence of moisture it means destruction of the conductor and its insulating covering by electric action.

III. LAMPS.

18. Arc lamps should always be guarded by proper lanterns to prevent danger from falling incandescent pieces of carbon, and from ascending sparks. Their globes should be protected with wire netting.

19. The lanterns and all parts which are to be handled should be insulated from the circuit.

IV. DANGER TO PERSON.

20. To secure persons from danger inside buildings it is essential so to arrange the conductors and fittings that no one can be exposed to the shocks of alternating currents exceeding 60 volts; and that there should never be a difference of potential of more than 200 volts between any two points in the same room.

21. If the difference of potential within any house exceeds 200 volts, whether the source of electricity be external or internal, the house should be provided outside with a "switch," so arranged that the supply of electricity can be at once cut off.

By order of the Council,
F. H. WEBB, Secretary.

Offices of the Society, 4, The Sanctuary, Westminster,

A THEORY OF FORESIGHTS.

THOSE of our readers who take any interest in rifle-shooting know that the small-bore Match rifle is fitted with a wind-gauge, on which is screwed a foresight suited to the vision of the marksman. These sights vary according to fancy, those most in use being the calliper, the solid bead, and the hollow bead or ring. By means of the wind-gauge, it will be understood that the bull's eye may always be in alignment with the sights.

I propose to call attention to certain optical advantages attending the use of the hollow bead which, I believe, are not generally taken advantage of.

The bull's-eye at the long ranges (800, 900, and 1,000 yards) is 3 feet in diameter, and the rings are made of different sizes. If we take one ·1 inch diameter and use it at all three ranges, the eye being say 4 feet from the foresight, the effect is roughly as follows:—

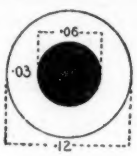
And it is obvious that, to obtain the perfect alignment necessary, the bull's-eye must be geometrically centred in the ring. As we get to the longer ranges, the bull's-eye fitting smaller, this geometrical centering becomes more difficult, a very slight error at 1,000 or 1,100 yards, in all probability, causing the shot to miss the bull; we should, therefore, use a smaller ring as the range increased.

In the use of rings of different sizes I have made the following observation:—

Let us take the range at 800 yards, the bull's-eye 3 feet in diameter, the distance of the eye from the foresight (in the back position with the back-sight on the heel-plate) 4 feet: the apparent size of the bull at the end of the barrel will be found to be ·06 inch. Now, if we take a ring ·06 inch, the effect produced is this,

a portion of the black being hidden by the diffraction fringe caused by light passing through the small aperture; it is inexpedient therefore, to use a ring the exact size of the bull, on account of the impossibility of detecting small errors. Although the measurement of diffraction fringe involves a somewhat extensive use of integrals and differentials, it may be practically measured in this instance, and becomes of very great importance. Let us take a ring double the apparent size, that is ·12 inch. (I may here state that gunmakers make these foresight rings accurately to the hundredth of an inch, and that Mr. Gibbs of Bristol has made me several for experimental purposes to thousandths; for all practical purposes, however, hundredths will be found sufficient.)

The effect is this (the diagram being drawn accurately):—



The diffraction fringe has all the appearance of, and acts in the same manner as, a solid ring, and, holding the bull exactly, the effect of a slight vibration or inaccuracy is this:—

By using such a ring the trouble of geometrically centering the bull is got rid of, and we may centre it optically; when the bull is seen perfectly it is centred, and the alignment of the sights is accurate. This centering cannot be mistaken, for the bull suddenly seems to expand to a great size.

On this theory, and with the data given above, the rings for the different ranges will be as follows:—

Eye 4 feet from Foresight.

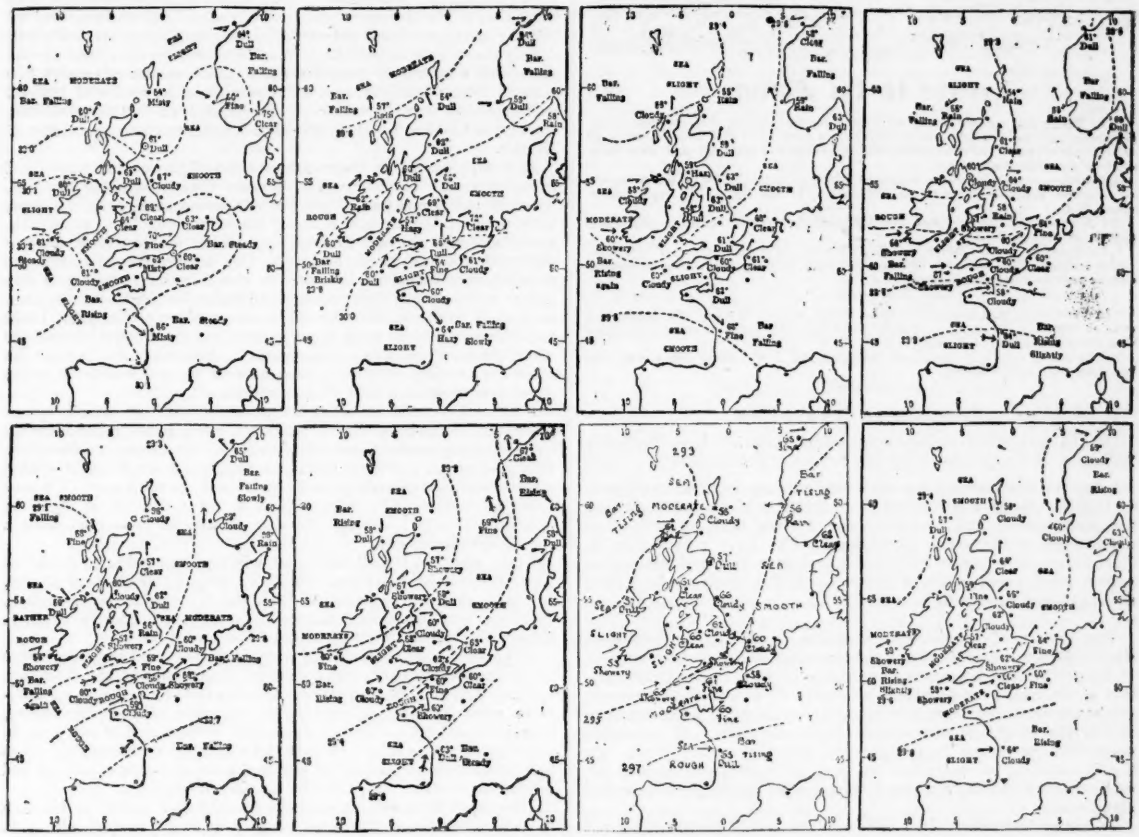
800 yards			·12
900	,,	·106 or roughly	·11 or ·10
(the smaller ring, I think, is preferable.)			
1,000 yards		·096 or roughly	·10 or ·09
1,100	,,	·087	,, ·09

Eye 3 ft. 6 in. from Foresight.

800 yards		·105 or roughly	·10
900	,,	·093	,, ·09
1,000	,,	·084	,, ·08
1,100	,,	·078	,, ·08

WEATHER CHARTS FOR WEEK ENDING SUNDAY, JULY 9.

SUNDAY, 2ND. MONDAY, 3RD. TUESDAY, 4TH. WEDNESDAY, 5TH.



THURSDAY, 6TH. FRIDAY, 7TH. SATURDAY, 8TH. SUNDAY, 9TH.

In the above charts the dotted lines are "isobars," or lines of equal barometrical pressure, the values which they indicate being given in figures at the end, thus—30·4. The shade temperature is given in figures for several places on the coast, and the weather is recorded in words. The arrows fly with the wind, the force of which is shown by the number of barbs and feathers, thus:— ——— light; ⟶, fresh or strong; ⟶, a gale; ⟶, a violent gale; ⊙ signifies calm. The state of the sea is noted in capital letters. The * denotes the various stations. The hour for which each chart is drawn is 6 p.m.

Eye 3 ft. from Foresight.

800	yards		·09
900	,,		·08
1,000	,,	·072 or roughly	·07
1,100	,,	·064 ,,	·06

I have tested all these rings in practice, and have found the theory true in all cases, the requisite conditions being, of course, good eyesight and good light; in very hazy, bad lights, nothing but the most open sights will allow the bull even to be seen; but in strong lights, with a heavy mirage, I have found the rings equally serviceable, the diffraction fringe hiding the image as effectually as it does the bull's-eye when it gets behind it. F. T. P.

THE STAMPING OUT OF SMALLPOX.—An important experiment, which is exciting the greatest interest in medical circles, is being made at Leicester, under the care of Dr. William Johnston, medical officer of health. There are more cases of noncompliance with the compulsory vaccination laws in Leicester than in all the other towns of the kingdom put together. Hence the past six months have witnessed very serious outbreaks of smallpox of a very virulent kind in the midlands, and at Leicester a very considerable number of cases have been imported from London, Birmingham, Nottingham, and other towns. Every one of these outbreaks was stamped out by the following method:—The corporation have powers under a local Act of Parliament compelling, under a heavy penalty, medical men and householders promptly to report the outbreak of infectious diseases, including smallpox and fever, the medical men receiving a small fee for each report. In the event of a case of smallpox being reported at the sanitary offices, which are directly connected with the fever and small-pox hospitals by means of the telephone, an officer at once communicates with the institution, the removal van is placed in readiness, and beds are prepared for the requisite number of patients. The medical officer of health and an inspector at once visit the house where the outbreak has occurred, explaining the nature of the disease to the inmates, and invariably succeed in inducing not only the patients to be removed to the hospital, but—what is of equal importance—in inducing every one who has come into contact with the sufferer, and every one living in the same house, to occupy quarantine rooms at the hospital. The telephone is again brought into requisition, and the removals take place with the least possible delay. Those in quarantine are provided with every comfort and means of recreation until the period of quarantine has elapsed, and meantime, the house where the outbreak occurred is thoroughly disinfected, at the expense of the local authorities.—*Times.*

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Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

TERRESTRIAL MAGNETISM.

[We have received an important communication from Professor Tromholt, of Bergen, of which the following is a translation.—ED.]

I beg to announce to you that next autumn I intend to propose to the Norwegian Government the formation of an establishment for investigating and observing the Aurora Borealis and other phenomena of terrestrial magnetism. With your permission, *M. l'Astronome*, in communicating this project to you and requesting your counsel, I will indicate in few words the motives which have led me to seek the realisation of this idea, in many respects a new one.

The theory of terrestrial magnetism is one of the problems which offers to natural science the strongest claims to examination. If science has not been able as yet to solve more than a small portion of the numerous problems belonging to this field of research, she has nevertheless been able at each step to prove more and more clearly the enormous importance of the phenomena of terrestrial magnetism in regard to the physical relations of our earth. By the discovery of a connection existing between these phenomena and events on the sun's surface, the question has acquired cosmical significance.

It would be useless, *M. l'Astronome*, to recall to you the great and important part played by the forces of terrestrial magnetism in the whole physical life of the earth, in their changing state, and their various periods, whether they present themselves in the perplexing play of the polar light, or whether they speak in the changing movements of the magnetic needle, or by their influence on the telegraphic network of whole countries.

No country in Europe, perhaps in the whole world, offers such favourable conditions as Norway for the study of these phenomena. Lying between the fifty-eighth to the seventy-first parallels of north latitude, she extends farther north than any inhabited country. She is nearer the seat of magnetic disturbances than any part of the European continent. The maximum zone of the aurora borealis crosses the north and north-west portion of the country. A continuous network of telegraphic wires extends from its northern and its southern extremity. The telephonic lines of Drontheim and Bergen are nearer the pole than any on the whole earth.

There is thus no country to which the duty of taking an energetic initiation in the study of the mysteries of terrestrial magnetism more directly presents itself.

Having recognised these numerous advantages, neglected till now, I have tried, during the few past years, to undertake, myself, and at my own expense, a part of the great work which still remains in this field, as yet scarce touched. In the autumn of 1878, I, for the first time, caused to be distributed in all parts of Norway, an invitation leading (Acte de la Société des Sciences of Christiana, 1880, No. 6) to the contribution of 839 observations of 154 auroras, taken between September, 1878, and April, 1879, from 132 stations, principally in Norway.

This result enabled me to prove that the aurora is a phenomenon occurring much more frequently, even outside the Polar regions, than is supposed at present, and that in a territory like Norway it seldom fails to occur even in a year of minimum frequency.

Counting from this modest beginning the plan has developed itself rapidly. Not only Sweden and Denmark, but Finland, England, Greenland, and Iceland, are now included in the field of observation. The complementary work relating to the observations of the winter of 1879-80, although long since finished, has not yet been published. This series is much more extended and complete than that of the former winter, and contains 1,600 observations of 269 auroras (boreal) taken from 357 stations. The material obtained during the winter of 1880-81 is much richer even than that of the preceding winter; it includes 5,200 observations of about 300 auroras taken at 675 stations. The result for the winter of 1881-82 will probably be still vaster. I correspond, at this present time, with from 1,000 to 1,500 observers of all the northern countries of Europe.

As a supplement to these observations of the aurora borealis, I have been able to get records at some fifty telegraphic stations, Swedish and Norwegian, of all disturbances occurring on the lines, with note of the time, the force, the direction, &c. From this work, material as vast and as astonishing as that from auroral observations has been obtained, the telegraphic disturbances showing themselves as frequent as the auroras; scarcely a day passes in Norway without some disturbance on the lines. As soon as I shall have received the observations of an entire year, I will publish them, with an atlas showing carto-graphically the telegraphic disturbances of each day, the auroras, and the storms observed the same day, noting time and place, as well as the extent of night cloud over the whole field of observation.

I have succeeded in constructing an apparatus, which, when placed in connection with a telegraph wire, notes graphically all the disturbing currents, according to their time, force, and direction. This Rheograph will soon be in communication every night with a telegraphic line, extending north and south, to a distance of about 1,400 kilomètres (870 miles) which is not employed at night in telegraphic work; during the day it will be connected with a special telephonic line.

I may add that I shall pass next winter (September to April) at Kautokeino in Finmark (lat. 69° 1′ N., long. 21° 0′ East of Paris), to make there, in connection with the Norwegian polar station, Bosekop, situated 100 kilomètres (62 miles) further north, observations with respect to the parallax of the aurora borealis.

Although I have received all possible support and encouragement from the Government and scientific institutions of the Northern nations, it is evident that an organisation so extended and so complete must end by exhausting the energies of one man alone, especially as I am obliged to devote the greater part of my time to other labours. It is also clear that an enterprise arising only from individual initiative cannot be pursued with as much exactness and perfection as when supported by a public institution.

[Professor Tromholt proceeds to describe the nature of his plan in so far as it depends on such support as he hopes to obtain from the Swedish Government. A magnetic institution is to be established at Drontheim, and will contain all suitable instruments, &c., &c. Professor Tromholt hopes to make it the central point for all observations of auroras in Europe. It will also take charge of all observations of telegraphic disturbances. "I am persuaded," proceeds Professor Tromholt, "that an observatory such as this will be able to render important services to science, and I hope that the realisation of this idea will be received with joy by the scientific world."]

I address this communication to you, *M. l'Astronome*, and to other recognised authorities in this department of science, that the question may be submitted to your judgment. I beg that you will communicate to me your opinion of the merits of such an institution, so that the weight of your name may help to support me in my efforts to realise my project. If (which I do not doubt) the idea should meet with your approbation I shall also be much obliged to you if you would be kind enough to give me your opinion and advice as to the organisation of the Institution.

Hoping to be honoured by an early reply, which may be expressed in any language you may prefer, I have the honour to be, *M. l'Astronome*, &c., &c.

Bergen, *June*, 1882. SOPHUS TROMHOLT.

FIGURES OF FLUID-MOTION.

[463]—I hope you, like your renowned neighbour, *Mr. Punch*, keep a regular staff of young men, to whom you turn over, like Mr. F.'s aunt to Clennam, all the crusts of your daily toast. For you, like all men of the Fifth Imperial potentiality (you like these fine terms, Sir—so easy to read out loud, and so full in the mouth, you know—"scientifically wrong and mathematically to be misunderstood," as you beautifully express it in the gorgeous shine of "our" KNOWLEDGE's first-page); you, Sir, I say, are doubtless as much toasted as you care for. Let the young men, then, make a

note of one of the simplest and prettiest demonstration of the still-hidden laws that govern the movements of "liquids in motion," and tell us all about them.

It is this: Into a two-pint porcelain mortar, break a (really) new-laid egg, and see to the proper levelling of the mortar, then procure a "Double Dover Egg-beater," and add a tea-spoonful of finely levigated white sugar. Now drive the egg-beater, first slowly and then extremely fast, and after a few minutes you will get your reward; for you will see a lovely illustration of some of the exquisite figures in moving fluids which the treatise of Mr. W. F. Stanley has so exquisitely taught to us, although I don't think he has, in any instance, observed the charming demonstration here named. The bowl is of full golden, shining bubbles of softest look and most tender lines. It is not one of my gifts to be able to lucidly describe all that I have seen in this homely experiment. But, after catching your new-laid egg, let one of "your young men" tell your readers all that this humble pen of mine fails to narrate, and, if he can, tell us "how it is done."

A "double," not a "single" beater is desirable for producing the varying shapes that dance in this golden tide. In proportion to the rapidity of the revolving arms do the shape, size, and behaviour of the changing lovely forms and figures follow. At a high velocity, the outline of a heart shows itself, the regular "Valentine" heart, none of your lumpy and unromantic anatomical hearts. It is impossible (but, doubtless, only so to myself) to reveal the life and sensitiveness of these figures, and their strange mobility. To me, they seem to set before us a science door through crack of which we may behold a vista of fascinating and important truths, waiting the resolute hand that will draw back the reluctant gate, that we may enter in. Alas! that hand belongs not to your faithful servant,

ROBERT ELLIS,
Raven Spring, Mitcham.

PROF. HUXLEY AND MARCUS AURELIUS.

[464]—Amongst the "pithy sayings" which the writer of the Review in KNOWLEDGE on Prof. Huxley's "Science and Culture and other Essays" justly considers as well worth quoting and remembering, there is one in particular to which I wish to draw attention. Prof. Huxley says, "Do what you can to do what you ought, and leave hoping and fearing alone."

Probably in all ages the thought of doing that which ought to be done, without hope and without fear, has been common to great minds; but I believe that it is to the Stoics, and to Marcus Aurelius especially, that we owe that noble lesson. I quote in full, and it can scarcely be too often quoted, one of the passages from "The Thoughts of Marcus Aurelius," which most resembles the modern rendering of Prof. Huxley.

"If thou workest at that which is before thee, following right reason seriously, vigorously, calmly, without allowing anything else to distract thee, but keeping thy divine part pure, as if thou shouldst be bound to give it back immediately; if thou holdest to this, expecting nothing, fearing nothing, but satisfied with thy present activity according to nature, and with heroic truth in every word and sound which thou utterest, thou wilt live happy. And there is no man who is able to prevent this." E. C.

IMITATIONS OF VOLCANIC FORM AND ACTION.

[465]—In a letter of mine to the editor of the *Athenæum*, some years since, I described a mode by which, accidentally, I had produced a surface similar to that presented by the moon. Having nearly filled the basin of a large Arnot stove with sand, I poured the water, for the purpose of moistening the atmosphere of the room, upon it till it became saturated sand mud; when the stove got hot, an effect similar to that described in the pages of KNOWLEDGE, with regard to the plaster of Paris, was produced. But I found that I could also realise miniature volcanoes, craters, and eruptions, by pouring a moderate quantity of water in at the side of the iron basin when the sand was dry and the stove excessively hot. W. CAVE THOMAS.

THE POTATO.

[466]—With regard to "Farmer's" "five experiments," I have to inform him that his results do not now surprise me, for he should so *pluck* the blossoms that all rising sap may be checked, throwing it back into the roots—*e.g.*, increasing the weight of tuber.

"Farmer" still adheres to his opinion about the "frosted potato." If "Farmer" will inform me how to reconvert starch once formed into sugar back again to starch, then, and not till then, can I believe that it is the "frost going out which makes the change." However, I should like to point out to "Farmer" that the alkali of the wood-ash, besides the perfect manner of preparing the food, might, and does, make an unpalatable potato palatable. Who can eat a potato cooked in the ordinary way after a glorious "spud" (so-called)? I cannot. Living all my life in the North, and amongst farmers, I was well aware of "Farmer's" remarks on the "large quantity of decomposing matter," therefore I made that "as an undeniable statement." The results obtained on the sandy ground are remarkable, but nevertheless true.

Farmer (329) asked me to give him my authority *re* "peat." I gave it, but in letter 405 he again says my statement is nonsense. I prefer to take "Biedermann's Centralblatt für Agricultur-Chemie" account of a genuine experiment before "Farmer's" opinion to the contrary. It would have been otherwise had "Farmer" been able satisfactorily to show that I had been talking nonsense by bringing forth data to prove my statements incorrect. "Farmer" has reminded me of a saying of Agassiz's through most of this discussion, "that whenever a new and startling fact was brought to light in science, people first say 'it is not true,' then that 'it is contrary to religion,' and, lastly, 'that everybody knew it before.'" Not that my statements are so very new or startling, for I believe that the question, whether blossoms or no blossoms yield the best results, has been before agriculturists for the last forty years at least. It seems a pity that it takes "Farmer's" body so many years to solve such a simple problem. F. C. S.

TOBACCO AND CONSUMPTION.

[467]—After a talk on the subject with me, a friend, who had one of his young chickens very ill with the "gapes," thought he would try as a last resort the influence of tobacco smoke on the disease.

Placing the chick under a bell glass, he blew the smoke from his pipe into it until the fumes had made the chick unconscious; then, removing the glass, he allowed the youngster to come round, it exhaled smoke for a second or two and was sick, but no symptom of gapes has been seen since and the bird is as well as ever it was.

If Koch or any other leader in germ research would try in a similar manner the effect of smoke on animals, now under their treatment, we might soon hear if this cure would hold good with respect to animals suffering from the inoculation of tuberculous matter; it would, in all probability, be necessary to make the animal unconscious many times before arriving at satisfactory data on the subject.

Since I first began to inhale—and I have inhaled nearly every kind of tobacco, from New Orleans perique to the finest Turkish from pipe, cigar, and cigarette—I have never been troubled with a single chest complaint.

Would—as suggested by "W. O. Prosser"—some of our medical contributors, who have been abroad, please inform us if consumption is prevalent amongst the following nations:—Mexicans, and the nations of the South American Continent, the Southern States of America; and if the custom of inhaling is carried on further north than New York in the colder parts of North America? We might in this way gradually find out whether the influence of tobacco inhalation has the effect of warding off or destroying consumption irrespective of climatic influence. P. C. S.

A VERY COMMON MIND TROUBLE.

[468]—The following, which appeared in the *Manchester Courier* of the 7th of this month, will, I trust, not prove inadmissible to the columns of your magazine:—

"In a large proportion of instances persons who live sedentary lives, and labour habitually with their brains to the neglect of the other parts of their bodies, suffer more or less annoyance from a mind-trouble, which, under favouring conditions, may readily become a disease. It consists of an uncontrollable and all-pervading feeling of doubt—not suspicion in the technical sense of that term, but doubt as to the commonest facts and acts of ordinary life and experience. At first the victim of this strange distress tries to compel his self-consciousness to rest. He resolves that he will not think of the act. It shall be performed instantly, or if it has been done it shall be forgotten. He goes away; presently the thought suggests itself that, after all, perhaps he has not accomplished what he intended, or that he has done it badly. He puts away this suggestion, but it returns with fresh force and overwhelms him. All sorts of evil consequences will, or may, ensue from his omission to lock that door or drawer, to turn off that gas-burner, or to place some ornament firmly enough on its pedestal! The demand, or impulse, to return and verify the underlying belief that all is well, will brook no denial. The creature of a craze like this may go to bed and try to sleep, but he will toss restlessly on his pillow, and at length he must—or he

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fancies he must—give in; and from any distance, at any pains, he returns, generally to find that his fears have been wholly groundless. Only those who have either themselves experienced this doubt, or repeatedly watched its growth or witnessed its effects in others, can imagine how terrible in itself, and what a prolific source of mental mischief, so seemingly small a matter may prove. It is, in fact, the first and warning symptom of many a grave, and—because neglected—afterwards incurable case of mind disease."—J. Mortimer Granville, M.D., in *Good Words*.

From observation, I think that the more thoughtful and intelligent persons are generally the victims, and indeed, after reflection, it seems natural it should be so. I should like to see this subject discussed in your columns, and to know the cause of it and the cure. W. M. M.

VARICOSE VEINS AND CYCLING.

[469]—I have head it said that medical men attribute the great prevalence of varicose veins to the use of bicycles and tricycles. I shall be glad to know if there is any truth in such a statement.

A TRICYCLIST.

THE DOUBLE STAR, CASTOR.

[470]—May I be permitted to suggest with regard to your reply to "Excelsior," on p. 83 of KNOWLEDGE, No. 35, that the fact that "no sensible change has taken place in the relative position or distance of the principal star and the 11′ one since the year 1823" (1829? H. and So's results are confessedly not very accurate here), tends rather to imply a physical connection than an optical one. I think you will find that the system is *not* "universally regarded among astronomers as being simply binary," because this small companion shares in the proper motion of Castor in the same way as the little pairs μ^2 Böotis or Σ 518 share in the P.M.'s of μ^1 Böotis σ^2 Eridani; all three being probably ternary. H. SADLER.

[F.R.A.S. wrote the reply in question, *not* THE EDITOR.]

MEDICAL WORKS.

[471]—"Enquirer" would be glad if any of the readers of KNOWLEDGE would inform him if any societies similar to the "New Sydenham," *i.e.*, societies which issue works on medicine and allied sciences to subscribers, and terms of subscription. MEDICUS.

AN ARTISTIC DOG.

[472]—I send you the following instances of what may be called artistic appreciation in a dog. My dog, on whose ambiguous descent "Skyey influences" have undoubtedly operated, pointed in a marked manner at a Japanese figure of a cat, in which the monstrous was blended with the life-like. It stood on the floor inside a shop, and I had to call the dog off just as I might had the creature been alive, not before the dog had evinced more than curiosity. Again, the same dog came unexpectedly upon a life-sized plaster bust of a man standing on the pavement. This aroused his attention, then surprise, and ultimately fear, or indignation; in fact, he barked in a way that reminded me of the late Frank Matthews as Quince, on meeting Bottom "translated."

It is clear that the dog's sense of form alone was appealed to, as neither object was coloured in at all the natural tints of the respective originals.—Yours, &c., J. POWER HICKS.

CHANGE OF COLOUR IN BLOWPIPE BEADS.

[473]—The colour of beads before the blowpipe is to be attributed to the luminosity, or vibratory motion, in the beads themselves, and not to reflected light rays.

In the case of the cooling gold bead, the molecules become condensed, thereby shortening their period of vibration, and causing the luminosity of the bead to run up the gamut of colour. So in the bismuth bead, the expansion of the free path of the molecules lengthens their vibratory period, and the light runs down the gamut.

Is it not possible that as various strings, when stretched, will pick out each its own note in a harmony of sound, and respond to it; so all matter shines—I have no other word—with the colour its vibratory period selects from the harmony of light?

AUG. W. ORR.

TELEGRAPH *viâ* BEHRING'S STRAITS.

[474]—Is it practicable to build a telegraph line as far north as Behring's Straits, and then a cable to America? There is one to Hammerfest, in Norway, as far north. Such a route would have the great advantage of a short cable, which could be multiplied indefinitely at little cost, and would, I presume, work quicker than a long one owing to the less induction. J. P.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—*1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 4. Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

LIEUT.-COL. ROSS. Your opinion that "KNOWLEDGE" is "the best twopennyworth that England ever had" is very gratifying; but I am obliged to decline further Green Beam and Blowpipe papers. Your views on the former subject I know to be incorrect: of the latter subject, so far as practical blowpipe work is concerned, I know nothing; but space will not permit of further articles on a subject in which few, I fear, take interest.—STELLARUM VOX. (1) The astronomical views of Tycho and Kepler were not without tincture of astrological fancies—natural enough in their time. (2) Astrology has no basis whatever in scientific facts; one cannot say it is not "a correct science," for it is not a science at all. (3) As held by the earliest Chaldæan observers, it may have been associated with a flat-earth theory; but the later Chaldæan astronomers recognised the earth's rotundity. (4) It is *not* "worthy of any credence whatever."—T. R. A. You do not seem to have noticed that an account of the mastiff Kepler's arithmetical feats is given in the series of articles on Intelligence in Animals. The work you mention is in hand for review.—R. ELLIS. Your poetical account of medical story suggests something rather "dreadful." If you will kindly consider that among our readers are many of tender souls and sensitive nerves, and so "do your spiriting gently," I have no doubt the story would be very acceptable. The returned proof is suggestive of a liveliness of imagination provoking to constant improvements. But printers have a pleasing way of introducing the letters, L, S, and D, in connection with such changes. If every column of KNOWLEDGE were treated that way, I fear it would be improved off the face of the earth. Thanks all the same.—ANXIOUS FRIEND. Exceptional care is taken about such advertisements as those. In the case you mention a sample packet was ordered, which proved quite unobjectionable.—JAS. DEAS. I do not remember anything in your communication which was similar to the very striking achievement of Caseneuve. As far as I remember, your account related only to parlour experiments, which may always be "got up" by the invention of simple signal codes. I think, however, it was the part suggesting animal magnetism which seemed to me to render your communication—which was rather long—unsuitable. Pray believe, however, that there was not the least intentional disparagement. Many other letters reached me on the same subject about that time. It requires rather careful discrimination to draw the line between what may be useful as scientific evidence, and what may not. Of course, H. M.'s evidence by itself may be open to exception. His friend may have been also a friend of the conjurer's. He himself may have forgotten some details. And so forth. But his amount accords well with the evidence given by Dickens. (I believe it was the same conjuror in both cases.) On the other hand, general accounts of thought-reading at evening parties, where the narrator "is sure there was no deception," are really of no value from a scientific point of view. An exact account of precautions to prevent deception should be given. (They were unnecessary in H. M.'s case, which depends solely on his own veracity and the trustworthiness of his friend.)—H. W. FAUCUS. There can be no doubt that when large masses of aqueous vapour are suddenly condensed into the cloud form, or large masses of cloud still further condensed by the gathering together of minute water-drops or vesicles into large rain-drops, electricity is generated and *vice-versâ*. It can also be shown experimentally that when oxygen and hydrogen in due proportion are converted explosively into water, electricity is generated. But the theory that lightning and thunder are caused by the explosive change of oxygen and hydrogen in the air into water is quite inadmissible. Hydrogen and oxygen nowhere exist in the air in the right, or anything like the right, proportions.—W. M. M. Thanks. Your letter shall appear shortly.—POSTCARD. You certainly have entirely misunderstood our notes "On Some of Our Critics." We wish *every* reader who sees any mistake, or thinks he does, to call our attention to it. We have been rather careful to show this. It is only the *way* of making corrections which is in question. Compare the two following corrections on the same subject, and you will see that while one is not only admissible, but useful, the other is not so well:—

(1.) "It appears to me that Dr. Wilson's use of the term quadruped as synonymous with mammal may mislead some of your younger readers, though, of course, no biologist could for a moment

misunderstand him. They might imagine that all four-footed animals are mammalian, or that all mammalians have either four legs, or (like man) four chief limbs."

(2.) "Dr. Wilson seems to imagine that all four-footed animals are mammals, and all mammals four-footed. Will he permit me to inform him that this is not the case. A crocodile has four feet, but Dr. Wilson will find, on further examining the subject about which he writes, that a crocodile is not a mammalian. Man, on the other hand, is a mammalian; but man cannot be said to have four feet, although during a portion of his existence he goes on all-fours. Nor can the whale, though a mammalian, be properly called a four-legged animal;" and so forth.

"Post-card" seems to think the writers I mentioned have complained to me of such patronising communications as the last. This is not the case. In only two cases (I introduced Mr. Grant Allen only by way of illustration—no one has yet explained botanical matters to him) have contributors had occasion to make what I consider the right sort of answer to letters of the kind. This they did, without any reference to the matter in writing to myself. But then came letters to me from the carpers, angrily denouncing me for poking fun (which I had not done) at writers so thoroughly acquainted with "the quartile character of two-fold duality." I may add that, in his imaginary illustration of Mr. Grant Allen "attacked by a critic," "Post-card" speaks as if the article on Hesperornis, &c. ("Birds with Teeth") in No. 2, were from Mr. Allen's pen. I feel able to assert the contrary, having, to the best of my recollection, written that article myself. To "Post-card's" conclusion, that only professors should correct the statements of professors, I entirely demur; that is just the sort of thing against which KNOWLEDGE sets itself,—the cant and humbug of specialism and officialism (specialism being, however, an excellent thing in itself and when free from cant, officialism necessary, and not bad when free from humbug).—IGNORAMUS. Cold weather of June may be capable of explanation, but it certainly has not yet been explained.—W. BENSON. We saw many other objections, quite as serious, to your theory; but the one we quoted came first to hand. How would you explain multiple tails, curved tails, side tails, cross streaks, and the like?—LIEUT.-COL. ROSS. Regret to find you have in so short a time entirely changed your views. You think that (1) KNOWLEDGE is diminishing in circulation (oddly enough, only three days before your letter came, "the largest weekly sale since we started" was announced to me); (2) that it ought to diminish in circulation; (3) that its so failing is due to the delay or omission of your articles (you do not say so in so many words, but imply it plainly enough). You point to the Exchange and Sixpenny Sale notices of volumes of KNOWLEDGE for sale as proof of this. The proprietors were flattering themselves that these notices resulted from their own announcements, though they want rather the first seven back numbers to make up volumes than to buy back complete volumes, which our new subscribers will willingly do. It may interest you to learn that all the volumes originally in stock have been sold, and that of others made up with numbers bought back, only seven remained in stock a few days ago.—A STUDENT. You rather surprise us; for Mr. Hume's view has been expressed even more strongly by medical men of experience.—SCIENTIA. Medical men would probable advise you not to sit up reading till one. Whether you can do so or not, without injury to your eyes or body, depends on circumstances which only those who know you well personally can be acquainted with, on your constitution, habits, eyesight, &c. If you neither drink nor smoke, and your food is usually plain, reading till one should not hurt you. But if you find it necessary to take strong coffee or tea, or to bind your head with wet cloths or the like, you may rest assured you are injuring yourself. I cannot myself advise you, quite in the dark as I am, nor allow anyone else to advise you through these columns, to what might be very good for A, B, C, F, and G, but very bad indeed for D (yourself, let us suppose). On the other point, I do not know; but I fancy that no societies meet on the dates you mention.—ALURED. The solution would run somewhat as follows, I suppose:—

$$\frac{1}{8}+\frac{4}{8^2}+\frac{6}{8^3}+\frac{3}{8^4}+\frac{1}{8^5}+\frac{4}{8^6}+\&c.$$

(1,4,6,3, recurring)

$$=\left[\frac{1}{8}+\frac{4}{8^2}+\frac{6}{8^3}+\frac{3}{8^4}\right]\left[1+\frac{1}{8^4}+\frac{1}{8^8}+\&c.,\ ad\ inf.\right]$$

$$=\frac{8^3+4.8^2+6.8+3}{8^4}\left(\frac{1}{1-\frac{1}{8^4}}\right)$$

$$=\frac{819}{8^4}\left(\frac{8^4}{8^4-1}\right)=\frac{819}{4095}=\frac{1}{5}$$

A. McD. Have forwarded your question to Mr. Grant Allen. I quite agree with you that the handkerchief shows on a London fog day, that it is better to breathe through the nostrils; but not that blowing the nose is a bad habit. The mucus does not act as an air filter. The irritating action of the matter which has been filtered out of the air causes the flow by which mucus is formed, and so the particles of carbon, &c., get in the way of being removed, preferably by the process you think objectionable.—W. SOUTHALL. Thanks; but the mistake is tolerably obvious. Our contributor wrote "Decoction or tincture of Cinchona," which was misprinted "Decoction of tincture," &c. He added on proof a marginal note, the last word of which was "quinine," but struck it out; unfortunately the last word seemed to remain unscored out and was taken as a correction for Cinchona. We have forwarded your note. There is, of course, no objection to your correction; on the contrary we thank you; still, you should hardly have supposed that anyone could be ignorant of the fact that "docoction of tincture of quinine," and "decoction or tincture of quinine" would equally be nonsense. A correction of an obvious "printer's error" would have been more acceptable than one implying a simply ridiculous, and, in fact, impossible blunder.—T. MORGAN. A number of correspondents object to the blowpipe articles, which for that and other reasons have now come to an end. But, as I learn from Lieut.-Col. Ross that you are in correspondence with him about a pyrological society, you can readily find other and better ways of announcing the plan to pyrologists.—EYE-WITNESS. We will convey to Mr. Allen and the author of "The Amateur Electrician" your suggestions about illustrations, but you must consider our small price and the absolute necessity that expenses should be kept down till the growth of our circulation has continued some time longer. We might at once do what you suggest if we made the price threepence per number. We prefer waiting till we can make such improvements without increasing the price. Already we have gone, in the way of expense (especially for original matter) far beyond what our price and present circulation justify, encouraging though the latter certainly is. It is *not* very encouraging to find our efforts in the way of cheapness altogether ignored as in your letter.—B. M., F.R.C.S. Some singular experiences in my own family circle confirm that view. But I do not care to cite them. —B. M., F.R.C.S., wishes to know where the mild tobacco mentioned at p. 79, letter 445, can be obtained.—ECONOMIST. Suggestions noted.—W. PEDDIE. Thanks.—J. R. C. Thanks; but we have given general solution for case of belts not crossing—understood in the problem. When they cross, solution is similar, so far as geometrical principles are concerned.—H. C. FLOYD. The geological structure of that spot is well known, and will probably be referred to by Mr. Harrison in the course of his papers.—F. COWLEY. No; you cannot multiply money by money. In the case of feet by feet, all you can say is that the number of feet in the length multiplied by the number in the breadth of a rectangle, gives the number of square feet in the surface. This is not multiplying feet by feet, however.—J. B. SHIPLEY. Thank you; but that way of doing the Fifteen Puzzle would not be of use to our readers.

CHEMICAL ANSWERS.

QUERIST.—Pure quartz does not lose weight by being heated alone (hydrous varieties lose water); in a current of steam, however, it slowly volatilises. If the mineral be powdered and fused, its *specific gravity* is altered; that of crystalline quartz is 2·6, after fusion it becomes 2·3. The same change takes place without fusion if the mineral is kept for some hours at a temperature of about 2,000° C. This lighter form is also found naturally, but only rarely occurs in crystals. When it does so it receives the name of "tridymite." These differ in shape from the crystals of quartz, and belong to another crystallographic system.

MEDICAL.

"A READER OF THE SAME" had better go where he can get plenty of exercise on elevated ground—the Yorkshire Wolds, the Sussex Downs, Firth Hill, &c.—avoiding valleys for residence. He should have a *pursuit*, whether it be geology, botany, or any other study, which will necessitate out-of-door exercise. He should clothe so as to avoid *chills*, and leave off all *spirits*. As to the Turkish bath, he should consult his medical man. A dose of Carlsbad salts every other morning will help him.

IMPROVEMENTS IN THE RUBBERS OF ELECTRIC MACHINES.—By G. J. Agostini.—Very fine charcoal-powder is rubbed up with petroleum to a tough liquid, and spread with a brush upon thin cotton paper. The surface of the ordinary rubbers is covered with strips of this paper. They are not hygroscopic; give a good development of electricity, and cleanse the disc.—*Wiedemann's Beiblätter.*

Our Mathematical Column.

EASY LESSONS IN THE DIFFERENTIAL CALCULUS.

No. III.

BY THE EDITOR.

THE plan we adopted in Lesson I. to obtain the differential co-efficient of the expression $\frac{1}{2}gt^2$ with respect to the variable t, will give us the differential co-efficient of many other variable expressions.

It may be well to try one other case, giving also an illustration of the value of such processes, before proceeding to obtain the differential co-efficients of various familiar functions.

Suppose, for instance, that

$$x = ay - y^2 \qquad \text{(i.)}$$

Increase x by Δx, and y by Δy; then

$$x + \Delta x = a\,(y + \Delta y) - [y^2 - 2y\Delta y + (\Delta y)^2] \qquad \text{(ii.)}$$

so that, substracting (i.) from (ii.),

$$\Delta x = a\Delta y - 2y\Delta y - (\Delta y)^2$$

$$\text{and } \frac{\Delta x}{\Delta y} = a - 2y - \Delta y.$$

Now make Δx and Δy infinitely small, calling them dx and dy. Then we get

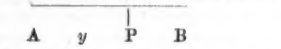
$$\frac{dx}{dy} = a - 2y - dy = a - 2y.$$

This process would be very cumbrous if applied to complex expressions. Therefore, the first matter considered in treatises on the differential calculus, is the determination of rules by which a differential co-efficient may be readily obtained. In the next paper on the subject I shall give some of these rules, without dwelling at any great length on the reasoning by which they are established. Much of this reasoning, indeed, would be beyond those for whose special service these papers are written. The advantage derived from the practical application of the differential calculus to problems not easily solved in other ways, will encourage the student to discuss after awhile the reasoning by which the rules of the calculus have been established. The great difficulty has hitherto been that this reasoning, coming before the student has learned the power of the calculus, has, by its length and complexity, prevented many from pursuing the study of the subject.

But even at this stage, it will be well to illustrate the application of the differential calculus.

Suppose we had this problem given :—

The length of the line AB is A ; *where must a point P be taken in*



order that the rectangle under AP, PB may be as great as possible?

Since A P is y, P B is $(a-y)$, and the rectangle under AP, PB (which call x) is $y\,(a-y)$; that is

$$x = ay - y^2.$$

We want x to be as great as possible. Now the differential co-efficient of x with respect to y, is the rate at which x increases with increase of y (from 0 to a) ; and so long as x is increasing, x is not as great as possible. We must find then when x ceases to increase, or when its rate of increase (or its differential co-efficient) is reduced to nought. Now we have seen that when

$$x = ay - 2y,$$

$$\frac{dx}{dy} = a - 2y.$$

Putting this equal to nought, we have the equation

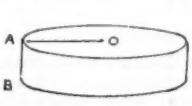
$$a - 2y = 0,$$

$$\text{or} \qquad y = \frac{a}{2}.$$

So that P must bisect AB, in order that the rectangle under AP, PB may be a maximum.

Here is another problem :—

AC is a cylinder constructed to fulfil the condition that its height AB added to AO, the radius of a circular face, is equal to a



fixed length A. *Required the height of the cylinder in order that its curved surface may be as great as possible.*

Put AB $= y$; so that AO $= a - y$.

Then the curved surface, which call x, is represented by the rectangle under the height and the circumference of a circular face. That is (representing the ratio of the circumference to the diameter as usual by π),

$$x = 2\pi\,(a-y)\,y = 2\pi\,(ay - y^2).$$

Here, as before, we must have the rate of increase of x with increase of y (or the differential co-efficient of x) *nought*. But if we went through the process for determining the differential co-efficient as above, we should readily get

$$\frac{dx}{dy} = 2\pi(a - 2y),$$

and the equation

$$2\pi(a-2y) = 0$$

gives

$$y = \frac{a}{2},$$

or the height of the cylinder must be equal to the radius of its base.

(To be continued.)

Our Whist Column.

BY "FIVE OF CLUBS."

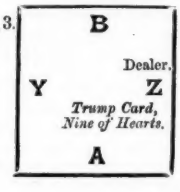
DEAR FIVE,—I have been studying Whist since KNOWLEDGE came out, having formerly played a good deal, but without much knowledge of the principles. I see you are working your way (for us learners) through the leads, play second and third in hand, return leads, and so forth. But I have a question to ask about matters you have not yet reached—illustrated in the following game, which forms the first example in Pole's lucid treatise on the "Theory of Whist." Will you kindly give some notes on the game. I do not express my own opinion as to particular points of play; but speaking generally, I may say that it seems to me the play on both sides (not of all four players, however,) is decidedly bad.—Faithfully yours, DEUCE OF HEARTS.

In the game referred [to by "Deuce of Hearts," as given by Professor Pole, the score, on which in reality the play would greatly depend, is not given. We assume that it is "love all;" but if it were *A B* love, *Y Z* three, *B*'s play would be better justified than it is under the assumed actual conditions, because then *nothing could save A B* (if honours against them) *but the possession of such cards, or at any rate such a long suit* by *A*, as *B* ought, under ordinary conditions, to hold himself,—to justify his signalling from five trumps one honour. On the other hand, if the score were *A B* four, *Y Z* three, *B*'s play would be about the best he could follow to *lose* the game. The play of *Y Z* also would depend much on the score. The game is as follows :—

THE HANDS.

A.
Hearts—10, 7.
Spades—A, K, 10, 9, 4, 3.
Diamonds—5, 4,
Clubs—K, 8, 6.

B.
Hearts—K, 8, 6, 4, 2.
Spades—6, 2.
Diamonds—9, 6, 3, 2.
Clubs—A, 7.



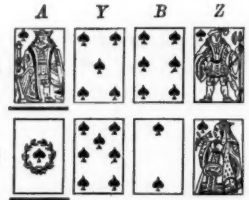
Y.
Hearts—A, Q, Kn.
Spades—8, 7, 5.
Diamonds—A, 10.
Clubs—Q, Kn, 10, 5, 3.

Z.
Hearts—9, 5, 3.
Spades—Q, Kn.
Diamonds—K, Q, Kn, 8, 7.
Clubs—9, 4, 2.

Score (probably) :—$\begin{matrix} A, B, = 0 \\ Y, Z, = 0 \end{matrix}$

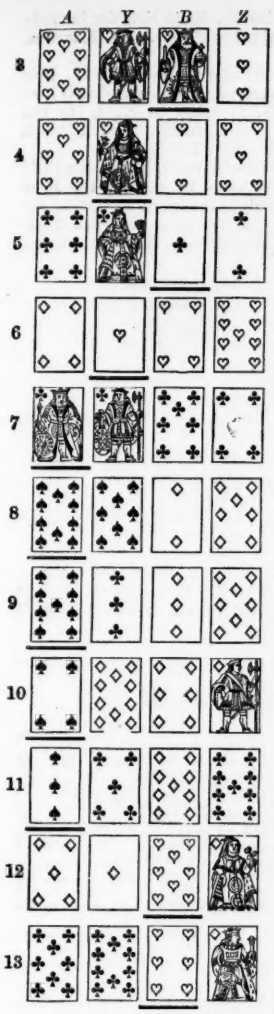
THE PLAY.

NOTE.—The card underlined wins the trick, and card below leads next round.



REMARKS.

1. *A* leads correctly (see leads). The Two not falling, he knows someone is signalling. *B* commences the signal. At the assumed score, *B* plays very badly in signalling, though if the lead had been with him he would have been right in leading trumps. When a player leads trumps he says to partner, "I am strong enough to play a forward game if you have



strength:" but when he signals he says, "I am strong enough to play a forward game even without your support, if you only bring in trumps. *B*, in this case, has only three probable tricks in trumps and one in diamonds.

2. *B* completes the signal; *A*'s suit is established. *Z* has no more spades.

3. *A* rightly leads his best trump. *Y*, from his own hand, knows that *B*, in all probability, has the King; for if *A* had the King he would not have led the Ten, and if *Z* had the King, besides the trump card, *B* would only have had small trumps, headed by 8, from which he would hardly have signalled. *Y* should therefore have put on the Ace. He gains nothing by this tenace being led up to, for if he had played the Ace, he would still have been sure of the third round of trumps as eventually played. Meantime he could have forced his partner in Spades.

5. Here *Y* plays very badly. It is useless to lead from a long suit at this stage of a game, in which strength of trumps has been declared against you. *Z* is not likely to attribute strength to *Y* in any other suit but Clubs, even if any harm could come, at this critical part of the game, from such a mistake. Leading Ace of Diamonds to save the game would have been sound play enough though this might suggest length in Diamonds: unless *Z* himself is very strong in them, as *Y* knows to be probable. But the force is the correct play to save the game. Of course *Y* knows that *Z* can trump, the trump card not having been played.

7. Even now, leading the Diamond Ace would save the game. (As a matter of fact it would do much more, as *B* holds four Diamonds; but of this *Y* cannot be assured. All he knows about Diamonds is that *A*, who has discarded one, is short in that suit; so that *B* is almost certain to have one at least). But *Y* blunders on with his long suit, on the chance that *Z* holds the King, and that *B* cannot ruff. *B* is much more likely, so far as *Y* can judge, to be able to ruff Diamonds than Clubs, for *Y* has two Diamonds and *A* is short in them, leaving at least eight between *B* and *Z*; whereas *Y* holds five Clubs, *A* from his discard certainly had at least three Clubs originally, while *B* and *Z* have already played one each, leaving only three to be accounted for between *B* and *Z*. Besides this, it is an even chance that *A* holds the King, not *Z*.

8, 9, 10, 11, 12, and 13. Of course nothing can now be done. *A* and *B* make five by tricks, against *Y Z*'s two by honours.

If at trick 5 *Y* had played properly, the game would have proceeded thus:—

	A	*Y*	*B*	*Z*		*A*	*Y*	*B*	*Z*
5.	S 10*	S 8	C 7	H 9	10.	S 9	H A	H 4	D 7
6.	D 4	D A	D 2	D K	11.	C 6	C Q	C A	C 2
7.	D 5	D 10	D 3	D Q	12.	C 8	C 10	H 8	C 4
8.	S 3	C 3	D 6	D Kn	13.	C K	C Kn	H 6	C 9
9.	S 4	C 5	D 9	D 8					

A B only making the odd trick.

NEVER BE TOO SURE.—"I once lost five by cards when I held Ace, King, and four small trumps, King and one Diamond, King and one Spade, and three small Clubs. It was my lead, and I led a small trump. Arrange the cards in the various hands so as to see how this happened, and could not be prevented after I had led. My partner had one trump only."—Drayson's "Art of Practical Whist."

* The best, to show partner he has entire command of the suit.

Our Chess Column.

BY MEPHISTO.

GAMES BY CORRESPONDENCE.

THESE games have an advantage not possessed by ordinary play, in that the exhaustive analysis each move is subjected to, can be recorded on paper actually as the game proceeds: and at the finish of the game, provided both players do the same thing, we obtain a more complete commentary upon a game than can be furnished by analysis, although the latter might be deeper. We herewith give such a game, and hope its perusal will be interesting to our readers.

Since the publication of the games between our Chief Editor and Chess Editor in our columns, two more games have been played "even:" with the result that each won a game. The first of the above games was a Guioco Piano, in which Chess Editor had the attack, and Chief Editor the defence. The former opened, as usually in this opening, with

1.	P to K4 / P to K4	2.	Kt to KB3 / Kt to QB3	3.	B to B4 / B to B4	4.	P to B3 / Kt to B3
5.	P to Q4 / P takes P	6.	P takes P / B to Kt5ch	7.	B to Q2 / B takes Bch	8.	QKt takes B / Kt takes KP

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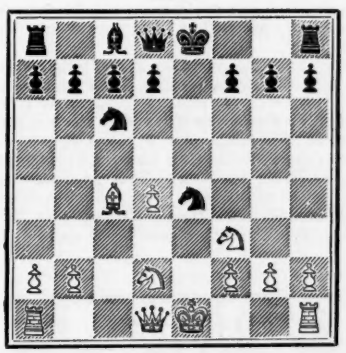


WHITE.

Here Chess Editor halted, as, for the sake of interest, he wished to examine into the merits of 9. P to Q5, which latter move is also mentioned in our analysis of the Guioco Piano, p. 442. The continuation given there is—

9.	P to Q5 / Kt takes Kt	10.	Q takes Kt / Kt to K2	11.	P to Q6 / P takes P	12.	Q takes P / Kt to B4

And for White's 13th move Q to Q5. The sacrifice of the Bishop here looks promising, and the following are a few results of its examination:—

13.	B takes P(ch) / K takes B		
14.	Kt to K5(ch) / K to K(sq)	Q to Q5(ch) (best) / K to Kt3	K to B3
15.	Q to Q5 / Q to Q2	P to KKt4 / Q to K2(ch)	P to KKt4 / Q to K2(ch)
16.	Castles KR / P to Q3	Kt to K5(ch) / K to B3	K to B(sq) / Q to K3 (best)
17.	Kt to Kt6 / Threatening	Castles QR / P to Q3	
18.	R to K(sq) /	P to Kt5(ch) / K takes P	
19.		R to Kt(sq)(ch) / K to B3	
20.		Kt to Kt4(ch) / K to Kt3	
21.		QR to Q(sq) /	

Being convinced that White could follow up that line of play, although in the last variation Black has a valid defence, Chess

Editor determined to make this the subject of further investigations, and the game proceeded 9. Kt takes Kt / P to Q4 10. KKt to Kt5.

This move is of an attacking nature. Black, of course, cannot take the Knight. P to KR3 would also be weak, on account of Kt takes P, and, if K takes Kt, then Q to R5(ch) would give White a fine attacking position. Chief Editor says in his note: "An ugly move; not, I think, in the books. The two Knights and the Queen threaten serious trouble on my King's side. However, I take the Bishop;" therefore 10. P takes B.

(To be continued.)

Note on the Steinitz-Winawer game, p. 86.

DEAR MEPHISTO,—I have been so backward with my work the last few weeks, owing to (what is unusual with me) ill health, that I have had no time to open a chess-board. Looking, now, over the singular game (for a great match) between Herren Steinitz and Winawer, on Friday, June 23, pp. 85, 86, I note that the ingenious variation suggested by you on move 16, while effecting a certain draw, if Black moves K to Kt(sq), gives White (at least, I think so) a win, if Black moves K to K2. Your continuation is—

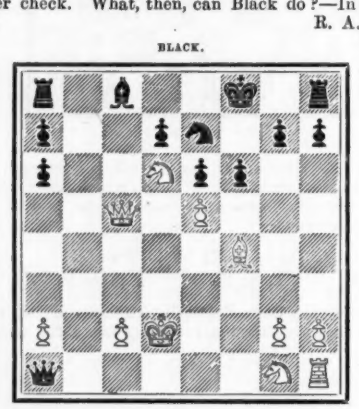
16. Q to KR5	16. Kt to Kt3
17. Q to R6(ch)	17. K to K2
18. Q to Kt7(ch)	18. K to Qsq
19. Kt to B7(ch), and perpetual check follows.	

But if after 18. K to Qsq
White plays 19. Q takes P(ch)
I do not see how Black can avoid mate in a few moves. Some of the variations are very pretty, but I see none in which mate is not forced. Is it not so?

About what follows I feel more doubtful, having only considered the suggested line of play in passing, when running through the game, as actually played. It seemed to me that if at move 14, White (instead of playing Kt to K2, giving the position shown in the diagram at p. 86) should play P takes P, and after P takes P, which seems forced, should play Kt to KB3 (sacrificing Rook), and after Q takes R, Kt to KR4, he wins by force. Black's best move seems to be to return his Q to QR8; but White can then play Q to KR5, and if Black Queen checks, move K to K2, safe from further check. What, then, can Black do?—In haste, faithfully yours,

R. A. PROCTOR.

BLACK.



WHITE.

For the better understanding of these excellent remarks, we give the above diagram of the position after Black's thirteenth move. We will first comment upon the second part of Mr. Proctor's letter, in which he asks whether, in the position given above, White would have done better to play

14. P takes P	14. P takes P

(14. Q takes P would be bad on account of B to Kt5)

15. Kt to B3	15. Q takes R
16. Kt to R4	

This certainly looks promising, and Black would probably lose if he now were to play Q to QR8. He has a better move at his disposal, however, which is

16. Q to KB8.

White may continue with

17. B to R6(ch)	17. K to Kt.sq

But after this there is nothing to be done of a determined character. For if 18. Kt to K4 Black replied with 18. K to B2, in which case White would do best to draw by Kt to Q6 (ch.), for should the Queen check on R5 Black can with advantage cover with the Knight. Or if 18. Q to KR5, Black can also play 18. Kt to Kt3, for in reply to 19. Kt takes Kt there follows 19. Q takes P (ch). 20. K moves. 20. Q takes Kt with an advantage.

In reply to 16. Q to KB8 White could also proceed with 17. P to Kt3 or B to K3, which would be met (we think effectually) by Black with 17. P to KR4. Finally, in reply to 17. Q to Q4. 17. Kt to K4. 18. B to R6 (ch). 18. K to K2. 19. P to B4. 19. K takes Kt. 20. P takes Kt. 20. K to K2, &c.

As regards the first part of Mr. Proctor's letter, he is quite right in saying that Black must accept the draw by moving the K to Kt sq. We were so elated at our analytical success in finding out the draw, that we overlooked that if Black plays K to K2 White will win. Resuming the position of the diagram, the variation proceeds thus:—

14. Kt to K2	14. Q takes R
15. P takes P	15. P takes P
16. Q to KR5	16. Kt to Kt3
17. Q to R6 (ch)	17. K to Ktsq
18. Kt to K8	18. K to B2
19. Kt to Q6 (ch)	

If Black now plays 19. K to K2, instead of K to Kt sq, White will win as follows:—

	19. K to K2
20. Q to Kt7(ch)	20. K to Qsq
21. Q takes BP(ch)	21. K to B2
22. Q to B3(ch)	22. K to Qsq or (*a*) or (*b*)
23. B to Kt5(ch)	23. Kt to K2
24. Q to R5(mate)	

(*a*) 22. K to Kt(sq). 23. Kt to Kt5(ch) and mate next move.
(*b*) 22. K to Kt3. 23. Q to Kt4(ch). (If K to B2, proceed as before.) 23. K to B3. 24. Kt to Q4(ch). 24. K to Q4. 24. Q to B4(mate).

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